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ORIGINAL DEPARTMENT.

Communications.

Vascular Nævi; with report of an interesting Case.

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Nævi are vascular tumors of uncertain origin; they are known by a variety of names, as mother marks, fruit spots, erectile tumors, telangiectasia, etc. It is not necessary that I should enter into detail of the numerous origins suggested by the unlearned and superstitious, but will endeavor to present the peculiar characteristics of those forms most generally met with.

Congenital vascular nævi are of rare occurrence in mucous membranes, especially if we except those cases which extend from the skin of the face to the mucous membrane of the lip. The case here reported, although not purely a congenital one, will be found to have pursued the opposite course, advancing from the mucous surface to the skin.

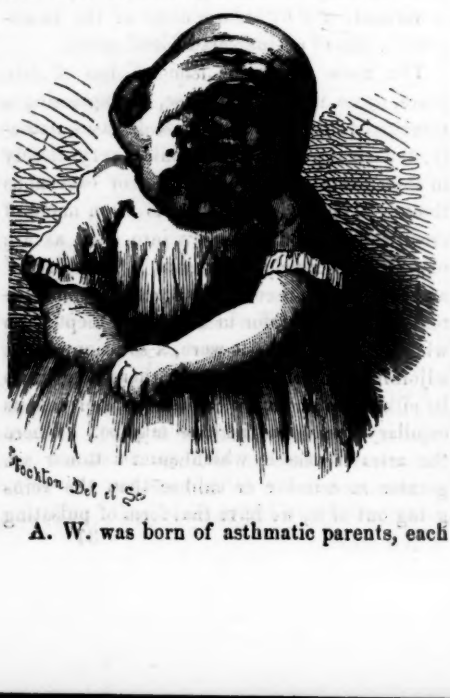
Telangiectasia in the skin is the well-known vascular nævus; it is almost always congenital, and consists of a net work of enlarged capillary vessels embedded in a delicate and partly undeveloped cellular tissue. I have said they are almost always congenital; they do, however, occasionally, commence in after life, and although they do sometimes take on a cancerous form, they are at first quite of a benignant nature, and it is only in individuals of a cancerous cachexia, that they become the seat of cancerous growth; and, under such circumstances, nævus has been taken for a special form of cancer, fungus hæmatodes cutis. As it is nothing more than an accidental complication, it cannot be considered an essential form of that disease.

As generally met with, cutaneous nævus presents the appearance of deep red, or bluish red stains of extremely various size and form. Sometimes the margins are elevated, and the tumors resemble cherries, plums, mulberries, etc. Those found upon the hairy scalp do not, generally, increase in size after birth, but on the other hand diminish, leaving nothing more than a thickened patch of "hypertrophied tissue." Where they do enlarge, the law which governs their increase, appears to be the reverse of that which is in force in the production of other structures, both normal and abnormal; instead of, as in those last named, a proportionate increase of vessels in the surrounding tissues, there is a decided decrease. And even the coats of the vessels, in large tumors, eventually give way, leaving shreds interspersed through the tumor, which is formed, not by enlargement of the blood-vessels, but of an increased blood space.

The name "erectile tumor," has of late years, come into general use, as expressing a principal fact concerning these diseases, namely, that many of them resemble very closely in their texture that of erectile or cavernous tissue. The division which is often made of erectile or vascular tumors into such as are named respectively, "arterial," "capillary," and "venous," is convenient, and to some extent well founded, for in all cases, except those wherein there is, as it were, a solution of the adjoining wall of the vessels, there appears to be either a predominance of arterial or venous capillary vessels, as the case may be. Where the arterial vessels which enter a tumor are greater in number or calibre than the veins going out of it, we have that form of pulsating

tumor known as arterial nævus, or aneurism by anastomosis. Where the veins predominate, we have a non-pulsating tumor, and the color of both forms is influenced by the character and size of the vessels constituting it. One important fact, especially in the treatment of nævi, is, that the vessels going to and coming from the tumor, are not enlarged to any extent, on either side of the disease, for they are not peculiar to individuals of a scrofulous cachexia, or those in whom the hemorrhagic diathesis exists; we can therefore with comparative safety "cut out such a tumor, if not of too large size, while it would not be safe to cut into one."

Various modes of treatment have been advised for extreme cases, simple incisions, or ligation, being sufficient for small ones, such as the seton, subcutaneous section, vaccination, and the injection of various astringents, which act by exciting inflammation, and the effusion of coagulable lymph, whereby the vascular net work that constitutes the nævus is obliterated. Others, such as caustics, the actual cautery, etc., excite a slough, and thereby destroy the morbid structure. In the case to which I am now about to refer, which occurred in the practice of my friend Dr. J. C. Richards, the disease advanced with such fearful rapidity as to render abortive all efforts to arrest it.



A. W. was born of asthmatic parents, each having a scrofulous diathesis, she was found to have inherited a tendency to asthma, which rapidly increased in intensity for a period of ten days or a fortnight, at the end of which time a purplish stain was noticed on the gums of the lower jaw, which advanced with great rapidity until the entire lower lip was involved and presented an ecchymosed appearance. A very few days elapsed until the entire carotid space, on the left side, was occupied by a large tumor, which caused the ear and cheek of that side to protrude greatly. On the opposite side a purplish tumor presented itself, which covered a considerable portion of the right cheek. A fact worthy of notice, although, perhaps not of any great pathological interest, is, that as soon as these bloody tumors appeared, all asthmatic symptoms disappeared. The blood contained in the tumors was evidently venous, as no pulsation could be distinguished at any time.

The child became exhausted from inability to swallow on account of the size of the tumors. Death finally resulted when it was eight months old, from ulceration of the nævus, followed by excessive hemorrhage. After death all appearances of the disease vanished, except a few ulcerated spots. The ambrotype from which the cut was taken, was procured shortly before death.

Medical Education.

No. 3.

Method, important as it is in the study of every science and every art, is—if it be possible—even more so in the study of medicine, where it is necessary to master so many collateral branches, in order to acquire the knowledge and the skill requisite for the recognition and diagnosis of diseases; for determining their nature, seats, and causes; and for the institution of the means best adapted for their prevention or their cure. To disregard all systematic progress in the study of the science and the art of healing, and to attempt to master these, as it were, *en masse*, as is too commonly done, even at the present day, is to waste much precious time in the acquisition of a confused and unconnected mass of facts,

principles, and precepts of very little, if any, real practical value.

We cannot, therefore, too strongly condemn the usual plan of sending a pupil direct from the office of his preceptor, where, perhaps, he has been but imperfectly instructed in even the elementary branches of medicine, to attend upon a full course of medical lectures, comprising some four or five a day, extending from the simplest of the institutes of medicine to the most difficult branches of special pathology and therapeutics, and arranged without the slightest attention, in the order of their delivery, to the relationship of one of the branches taught with either of the others.

To study medicine correctly and profitably, it must be studied methodically,—beginning with the most elementary of the collateral branches, and progressing methodically onward, until the entire course of medical instruction has been accomplished; not passing from any one branch until it has been mastered, to such an extent at least as shall enable the pupil to commence the branch next in order with such an amount of the requisite preliminary knowledge as will enable him to enter upon its study understandingly.

A very full, excellent, and systemic course of medical studies might, we think, be thus arranged:—1, Botany; 2, Materia Medica; 3, Chemistry; 4, Pharmacy; 5, Anatomy; 6, Physiology; 7, General Pathology; 8, General Therapeutics; 9, Special Pathology; 10, Special Therapeutics; 11, Medical Logic; 12, Medical Jurisprudence. The diseases of women and children being taught in connection with those of the male and of adults. After the sixth branch, Obstetrics may be properly introduced, and after the eighth, Surgery. We need not repeat here what was said in our first essay, in respect to the importance of the ninth and tenth branches being taught in connection with a properly arranged and sufficiently extended course of clinical instruction.

It is not necessay that each of the twelve branches we have included in our scheme of medical instruction should be studied separately, to the neglect of the other eleven. There would be no impropriety nor inconvenience in pursuing at the same time the study of the first, second, third, and fifth; the sixth, seventh, and eleventh, or the ninth, tenth, and twelfth. But it would be perfectly absurd to commence the study of pharmacy before the pupil knew aught of materia medica and chemistry; or of physiology, before he was acquainted with anatomy; or of pathology, whether general or special, without any previous knowledge of the normal condition, structure, and functions of the several tissues and organs of the body.

However extended our curriculum of medical studies may, at first sight, appear to be, it may be satisfactorily accomplished by a diligent pupil within a very reasonable period. To enable him, however, to do this, it is essential, either that the term of instruction in our medical schools be extended, or that the province of teaching medicine and the power of conferring the doctorate, and of thus licensing to practice as a physician, should be entirely separated from each other.

The latter, which is the most desirable improvement of the two, would enable the student to seek for knowledge in either of the branches comprised in the science and practice of medicine, wherever it can be best and most conveniently acquired. Thus a knowledge of botany could be obtained of a special instructor, during the summer or autumnal months; of materia medica, chemistry and pharmacy, at schools devoted solely to these branches, during seasons and hours unoccupied by any other branch to which the student can profitably devote his attention in connection with them; while all the remaining branches he would be at liberty to pursue in whichever of the schools or colleges they are best taught, and by the ablest instructors.

By allowing to the student such a freedom of choice, it is reasonable to expect that the facilities for the proper acquisition of all the branches which enter into a complete system of medical instruction would be multiplied; while by the competition it would necessarily excite among the teachers of each of these branches, the value and efficiency of their respective courses of instruction would be greatly increased.

In all the branches of medical education, oral instruction must always enter as a prominent and important feature. There is, perhaps, nothing better adapted to fix the attention of a class of pupils, and to impress upon the mind of each of its members clear views in respect to the existing state of knowledge upon any given subject, than skillfully prepared and well delivered lectures, especially when fully illustrated and recapitulated at regular periods by a series of examinations extended, in turn, to every member of the class.

In connection with the lectures on some, at least of the branches of medical science, an important improvement may, we are convinced, be introduced. It is to furnish the pupil, in all cases in which it is practicable, with the means of testing practically, or—in other words—of demonstrating for himself the facts' principles, and directions taught him by the lecturer.

In respect to anatomy, this has already been done in the dissecting classes, and to a certain extent in respect to special pathology, practical medicine, and surgery, in the clinical wards of our hospitals. The system may, however, be extended also to chemistry and pharmacy,—classes being organized for the purpose of repeating the various experiments and manipulations exhibited in the regular course of instruction, as well as to physiology and pathology,—in both of which latter branches, the student, by comparing diseased with normal structures, and subjecting them both to the test of the microscope and of the processes of organic chemistry, under the direction of an able and judicious guide, would be able to acquire an amount of useful knowledge and practical skill in professional investigation that could be obtained in no other way.

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DR. EVES, Surgeon to the Cheltenham General Hospital, England, has recently performed amputation above the knee with successful result, in a case of elephantiasis.

EVERY pound of Cochineal contains seventy thousand insects.

Illustrations of Hospital Practice.

PENNSYLVANIA HOSPITAL.

SEPT. 24TH.

Service of Dr. J. Forsyth Meigs.

(Reported by Mr. J. B. Hayes.)

Albuminuria.—Exhibited to the class on two previous occasions. (See REPORTER, Sept. 24th.)

It will be recollected that it was difficult to produce an evacuation of the bowels. The compound jalap powder, elaterium, castor oil and turpentine, were in turn administered.

His symptoms have improved. He passes now five pounds of urine in 24 hours, his bowels are regular, his cough better, and the tension of the skin of the abdomen and legs somewhat less. The penis was still greatly distended. The albuminous deposit continues less than it was.

Treatment.—Still upon the bitartrate of potash, with juniper berry infusion; also tr. ferri muriatis, xv drops three times a day. He also has a hot vapor bath every other night to promote the function of the skin.

Hypertrophy af Liver and Spleen—Probable Tuberculous Diathesis.—The patient was a man 42 years of age, by occupation a weaver, born in Ireland, and has been in this country 6 years. In 1855 he injured his back, and since that time has suffered from fugitive pains, with occasional cough, dyspnœa and debility, at times being confined to his bed. The swelling of his abdomen began last December, decreased, and returned last July. He was treated for ascites at one time out of the house. He was admitted to the hospital on the 12th of August. There was then no effusion into the peritoneum.

Inspection.—General appearance emaciated; abdomen enlarged and distended, especially in the hypochondriac and epigastric regions. The iliac and hypogastric regions being nearly natural in size, the enlargement of the abdomen assumes a barrel-shaped appearance; veins of abdomen enlarged and prominent.

By *palpation* the hand was conscious of an unusual enlargement in the upper lateral regions of the abdomen, produced by the presence of two large tumors. In the middle the hand could be depressed between the two tumors, and some inches below the umbilicus the abdomen was relaxed and supple.

There is marked fluctuation, which is not so distinct when one hand is placed on its edge in the median line of the abdomen. By percussing over the pubic region in the horizontal and then in the vertical position, the change from the recumbent to the upright posture, a moderate effusion can now be detected in the peritoneum. This did not exist two

weeks ago. These tumors, Dr. M. remarked, are two in number, are divided by a sulcus towards the left of the median line, and are, I have no doubt, the liver and spleen. The larger one sweeps from the left side of the epigastric region nearly to the crista ilii of the right side, percussion over it is quite dull and resisting, and clearly marks out its boundaries. It is evidently a greatly enlarged liver. The other tumor juts out from beneath the left false ribs, and extends downward in an irregularly oblong form to some inches below the line of the umbilicus. It is formed by a greatly enlarged spleen. Both tumors have smooth, rounded edges. They have to the touch, a somewhat doughy feel, without any thing like fluctuation, without the hardness of carcinomatous disease, and entirely without knobbed or irregular projections or protuberances at any part.

Examination of Chest.—Percussion under the clavicles shows the left to be of fuller tone than the right. There is but little difference in the upper posterior regions. *Auscultation* shows distinctly a somewhat harsh murmur, with some prolongation of the expiration under the right clavicle. On both sides the sounds are a little louder than in health; his emaciation and the pressure on the lungs from below account in part for this. In the lower lobe of the right lung, behind, there is well marked crepitation, especially toward the close of inspiration, and some dullness on percussion, as compared with the other side. There is no cough.

The action of the heart is natural. The apex beat is in its normal position, but so feeble that it is with difficulty that I can localize it. I believe he has no cardiac disease. His tongue is clean, soft, moist, red, and healthy; no inference can be gathered from it. His gums are unhealthy, but that is no unfrequent thing with hospital patients, whose gums, from neglect of the tooth brush, are often found soft and flabby. He is not jaundiced; he has epistaxis, and is costive.

The patient was here removed and Dr. M. remarked: This is an interesting case, and we must endeavor to form a correct diagnosis. It is important to know the cause of the enlargement of these organs. Their hypertrophy is proved in various ways—by inspection, by palpation, by percussion, by the enlargement of the superficial veins, and the presence of some peritoneal effusion. His lungs are not in good condition, and though he has no cough, I believe that he is tuberculous. He has a fine crepitation over the lower two-thirds of the right back. That crepitation I am confident is not caused by pneumonia, pleurisy, or bronchitis. In my opinion he has a tuberculous deposit in the right lung, and this lends assistance in explaining the cause of the enlargement of the liver and spleen. We have here a lardaceous liver and spleen, or bacony, or albuminous, as it has been called. It is not cirrhosis or cancer, for the tumors are smooth. There are no nodosities; they are doughy to the touch, and feel almost as though they might pit under pressure. There are other reasons against the presumption of cirrhosis or scirrhus; in both of these diseases the liver is harder, the obstruction to the portal circulation is greater, and there is usually a larger amount of ascites early in the disease This man is not jaundiced; on the contrary, he is pale. The enlargement here depends, in all probability, on an albuminous deposit into the inter-lobular areolar structure. The liquor sanguinis of the blood is effused, the serum is absorbed, and the fibrin remains behind and contracts. It is what Dr. Budd calls scrofulous enlargement. He has the clubbed nails, which are a sign of tuberculous disease. I thought at first that he might have leucocythæmia, but the number of white corpuscles in the blood is not increased. The blood has been carefully examined under the microscope on three occasions.

Treatment.—He has good diet. I have put him on the muriate of ammonia, 7½ grains three times a day. This is recommended by Dr. Graves of Dublin, who reports several cases of enlarged liver cured by muriate of ammonia, when there seemed but little hope of recovery.

He also takes tr. ferri. mur. xv drops three times a day.

Chorea —This occurs in a seaman 35 years of age, whose story is, that it began to manifest itself immediately after a great fright, five weeks ago. He was in a coasting vessel which lost its anchor during a gale. He says that, previous to this, he has always been healthy. I can find in him no deranged function—no symptom of any other disease save this extraordinary one—chorea. Of the exact nature of this disease, we know but little. It appears to be a functional derangement of the nervous centres, connected in some way with a faulty state of the blood, which, as I stated in a previous lecture, I have nearly always found thinned and hydræmic. By Romberg it is classed among the spinal spasms. Dr. Carpenter, with correctness, it seems to me, locates it in the upper portion of the cranio-spinal axis, arguing that the influence exerted upon the irregular muscular movements by emotional states, and yet more, the quiescence during sleep, show that not the true spinal centre, but rather the emotional and volitional centres must be the parts chiefly implicated.

It has been very generally supposed that there was some essential connection between chorea and rheumatism. This is the opinion of many of the best English writers, and amongst the French, is M. Sée, whose essay, published in the Memoirs of the Imperial Academy of Medicine, is a very full and

able one. In my own experience, I have not observed this connection, and MM. Rilliet and Barthez, in their last edition, are disinclined to admit any necessary or constant relation of the kind. This patient, you may observe, has a curious control over the convulsive movements. By an effort of will, they are very much calmed, but return when his attention is withdrawn. His appetite is good, and he is able to feed himself.

Treatment.—He is upon the use of arsenic, given as recommended by Wilson—liq. potas. arsenit. gtt. v, tr. zingiberis, vin. antimonii, aa. gtt. x. He has cold shower-baths daily. Dr. Todd recommends cold affusion, two or three times a day, in very high terms. He states, indeed, that he has not unfrequently cured the disease in this way alone, without the use of drugs.

SEPTEMBER 28TH.

Chronic Diarhœa.—Dr. M. first introduced to the notice of the class, the case of chronic Mexican diarrhœa, (see REPORTER, of Sept. 17,) in which he had suspected the development of tuberculosis. The other patient, shown on a previous occasion with this person, has been discharged, cured. This patient has now four or five discharges a day, watery, flocculent, mucous, and unhealthy. These are each quite small in quantity—not over a wine-glass full. Dr. M. called attention to this as worth observing; for the prognosis often depended on the amount of the discharges. If they are numerous, and at the same time copious, they become exhausting; but if small, they are not of so much consequence. He had known infants, who nursed abundantly, to have ten or twelve a day for months. The tongue was smooth and shining, with linear ulcerations and layers of exudation-matter scattered over it; the lips and angles of the mouth were excoriated. This aphthous sore mouth was a common accompaniment of tedious ailments, and was, in this case, connected with chronic inflammation and ulceration of the colon. Dr. M. remarked: I have no doubt now that his diarrhœa is connected with tuberculosis—that there is a tuberculous deposit in his alimentary canal. Since you saw him last, there have been more marked changes in his chest. I told you that he had dullness of percussion, under his left clavicle. He now has harsh respiration, prolonged expiratory murmur, and moist rhonchus. He has hectic fever, and his husky voice shows a thickening of the covering of the vocal ligaments; he has erythematous laryngitis, with probably some ulceration.

I do not think his disease *began* as tuberculous. I suppose that at first he had simple climatic diarrhœa; and in consequence of the lowering of his health, and of imperfect assimilation of food, he was placed in a position to contract tubercular disease.

The milk diet of this patient was put aside on account of his unwillingness to continue it, and a meat diet substituted. He now takes a powder every six hours, composed of—

℞. Kino,
Bismuthi subnit. āā, gr. x.
Opii pulv. gr. ½. M.

Syphilitic Periostitis —This patient, a well developed middle-aged seaman, had a chancre eighteen months ago. Two months since he was attacked with a painful periosteal inflammation on the front of the tibia, which still continues. He has also a chronic inflammation of the left testicle, which he has been treating himself. In addition to these symptoms, he is suffering with acute rheumatic inflammation, marked by redness and swelling of the left wrist, and with some pharyngeal soreness and redness, for which latter conditions he came into the hospital. When he entered, three days since, he had a smart fever, increasing at night, and was suffering a good deal.

Treatment.—He was ordered to bed, and to have a powder containing one grain of the antimon. sulphuret præcip., with five grains of the pulv. opii et ipecac., three times a day. The tibial inflammation was dressed with the following ointment:

℞. Ungt. hydrarg.,
Ext. belladonnæ, āā ʒi.
Axungiæ, ʒvi. M.

The wrist was directed to be wrapped in soft flannel, and this to be covered with oiled silk, Dr. M. remarking that for these acute rheumatic inflammations of the joint, nothing gave greater or speedier relief than the use of some impervious covering like the oiled silk or india-rubber cloth.

The preparation of antimony, selected in this case, Dr. M. considered one of the best. He preferred it to tartar emetic, as much safer in its administration, and related some cases occurring under his own observation, where very small doses of tartar emetic (1-12th gr. repeated twice in one case, and ¼th gr. in another) had brought on the most alarming symptoms.

Periostitis—A Sequela of Typhoid Fever.—This patient, after convalescence from typhoid fever, was attacked with acute rheumatic pain in both shoulders, and had pericarditis, with some effusion in the pericardium. He went out apparently well, and returned after a few days with severe periostitis of the tibia. He denies having any syphilitic taint, and states that a few days ago he got his feet wet, during a rainy day. If we can believe this, we must attribute this affection to the effect of cold on a sub-

ject not yet entirely recovered from his previous illness. We have lately seen double crural phlebitis as a sequela of typhoid fever; here is another rather unusual sequence of that disease.

Treatment.—The same ointment as prescribed in the last case, and ½ gr. of golden sulphuret of antimony, with five grains of Dover's powder, three times a day.

Recent Specimens Tuberculous Disease of Lungs and Peritoneum.—These were obtained from a patient who had died a few days after being removed from the house. The disease was an obscure one, and its study had been to Dr. M. very interesting. The patient was removed from the hospital but two days before he expired, by his son, who, with an amount of common sense not often met with in the ignorant and poorer classes, acceded to the request for a post mortem examination. It was with great satisfaction that he was able to inspect and to exhibit these specimens.

The history of the patient was as follows: He was 66 years of age, born in Germany, and a ragpicker; was admitted on the 12th of August, and died on the 26th of September. His disease was of six weeks duration before he came here. He was ill, altogether, about three months. He was supposed to have been threatened with phthisis, as his son informed Dr. M., twenty years before, but recovering, had gone on with the occupation of ragpicking.

When admitted, he was feeble, emaciated, and the following physical signs presented themselves:

Percussion over the front of the chest was too sonorous; it was abnormally clear; behind, it was dull over the whole *right* scapular intersperse; *left* not quite so dull; lower, posterior region quite sonorous.

Auscultation revealed almost nothing, at least no alteration sufficient to build a diagnosis upon. Behind, over the *right* scapular interspace, a mucous rhonchus could generally be heard; this was the only material alteration. He coughed very little; indeed, he always denied having any cough, but was heard occasionally to cough slightly; he had no expectoration, no pain in the chest, no dyspnœa, and could take a deep inspiration without difficulty. His respiration was a little hurried during the last two weeks of his life. There were no abnormal sounds of his heart. Percussion showed that his liver was somewhat diminished in size. The abdomen was rather flattened, except in the centre, where there was a slight rounded fulness. This, however, was but slight, and was limited to the central portion.

The sensation imparted to the hand was peculiar, and led him to believe the patient had had peritonitis and agglutination of the convolutions of the intestines; it was a doughy, semi-hard, semi-elastic feeling. The abdomen was not dull; there was an obscure sonoriety all over it. When pressed, the patient always complained of obscure pain, and evinced discomfort by a grimace. There was no effusion or fluctuation. He had a little slow fever, which augmented toward the last, with scarcely any appetite. He had a shining, dryish tongue; stools generally pretty free, one or two a day, softish, with sometimes an absence of bile.

Huxham's tincture of bark and iron was pretty much all the medicine he had. Under this his appetite improved for a few days, and then fell away.

At the post-mortem examination a very extensive old tubercular deposit was found in both lungs. The upper two-thirds of the right lung were nearly solid, containing numerous whitish gray tubercles, intermingled with a large amount of black pigmentary deposit; the lower lobe also contained a good deal. The left lung contained, in its upper half portion, the same grayish deposit, with black pigment. It was hard, and creaked under the scalpel. The lower portion was perfectly healthy.

The peritonæum was found to have been chronically inflamed; the whole anterior layer was greatly thickened by a fibrinous deposit fully one-third of an inch in thickness. It was adherent to the omentum, and the small intestines were glued together in every direction. It was curious to observe how, with this condition of things, the peristalsis of the bowels went on so well as it had done in this case. Dr. M. had rarely seen cases of peritonitis in which the peristaltic movement was not more or less obstructed; and in some forms of local peritonitis, especially in those attending typhitis, the obstruction had been complete, so much so, indeed, as to cause obstinate, and even stercoraceous vomiting.

Pieces of the fibrinous exudation were exhibited to the class, dotted over with gray granulations of tubercle. This fibrinous exudation was the result of slow inflammation, set up in the peritoneum, in the opinion of Dr. M., by the deposit of tubercle upon that membrane.

The consolidation of the lungs here was not the complete solidification of congestion, or atelectasis pulmonum. Although hardened, the lung tissue contained a large amount of air; on squeezing it, a highly aerated fluid exuded, showing that a great many pervious air vesicles were interspersed throughout the consolidated lung: this fact explained the sonorousness upon percussion.

This case had excited a great deal of interest in his own mind and in others who had seen it, on account of the uncertainty of the diagnosis. He was at first inclined to think it was tuberculous peritonitis, but then the advanced age of the patient, and the

absence of well marked physical signs in the chest, seemed opposed to that conclusion. On the whole, he had supposed it might be a cancerous disease of the peritoneum or of the pancreas. His first impression turned out to be correct.

Dr. Da. Costa, who had seen the specimens, supposed that the peritoneal disease might be the result of slow inflammation with successive layers of exudation, caused originally by a deposit of tubercles of many years back; and this was, indeed, the most reasonable explanation of the phenomena.

The pulmonic tuberculous exudation had undergone the process of cure described by Rokitansky—a conversion into obsolete tubercle, with large deposits of black pigment matter. This black pigment was very frequently found surrounding old tuberculous deposits. It occurred, also, in old people, independent of other disease, but never to the extent witnessed in this patient.

This case was also interesting and important in another view. It should make us very careful in giving an unfavorable prognosis. The physician was not to pronounce a patient doomed to death unless well assured of his facts. This patient had lived for years, apparently well, with only a portion of one lung free from tuberculous deposit.

Dr. M. related, in this connection, several instances of remarkable tenacity of life, from his own observation, where the physician might have been tempted, from appearances, to despair of recovery. He also referred to two memorable instances in the great house of Guise, one of the founders of which, the Count d'Aumale, was left for dead, with many wounds, on the field of battle, but who recovered, and whose son, then Count d'Aumale, and afterwards the first Duc de Guise, formed the subject of the famous story of old Ambrose Parè. This young nobleman was pierced through the head by the spear of an English horseman. After being pronounced hopelessly wounded by the surgeons who first saw him, old Parè extracted the spear, and the Count survived to lay the foundations of the noblest and most renowned of the princely houses of France.

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JEFFERSON MEDICAL COLLEGE, PHILADELPHIA.

SEPTEMBER 28TH.

Clinic of Dr. Gross.

Tertiary Syphilis.—This patient, a female, had presented herself on a previous occasion with two ulcers upon the leg, having hardened and excavated edges, and covered by a persistent diphtheritic slough. They had existed two years, and were surrounded now by a dark red patch of inflamed skin. An application had been made to the ulcers, of the acid nitrate of mercury diluted with two parts of water. The ulcers had put on a better appearance; they were red and granulating. She had also been put under the use of iodide of potash, ten grains, with corrosive sublimate, one-tenth of a grain. She comes back now more comfortable in every respect.

Taking into consideration the excessive obstinacy of these sores, the peculiarity of their edges, the enlargement of the tibia, the scars upon her forehead, and exacerbations of pain at night, Dr. Gross had no hesitancy in coming to a conclusion as to the nature of the disease; and this was corroborated by the effect of the treatment: there had been a marked improvement. It was a specific ulcer,—tertiary syphilis; there could be no mistake about the diagnosis.

She would continue the use of the iodide of potash, this was a great remedy for dislodging the poison of the disease; ten grains was a good average dose. Experience had shown that it acted specifically, in what manner, nobody could tell. By combining with it the bichloride of mercury a much more decided and permanent impression is made than in any other way. He had not the slightest objection to touch the woman's gums and keep them sore for weeks; but he would have a decided objection to salivating her. Her impoverished system should be built up by a nutritious diet.

Dr. G. also prescribed an ointment.

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This was a stimulating and slightly cicatrizing application. It should be applied to the ulcer by patent lint, three times a day.

Equal parts of alcohol and tr. iodine should be painted on the surrounding structures.

Exstrophy of the Bladder; Congenital Malformation.—This was a boy 3 years old, one of those melancholy cases which come into the world with an infirmity of an incurable character. A reddish tumor was visible just above the pubis; this was the posterior wall of the bladder, its anterior wall, together with the straight muscles, being wanting. Enumerating the parts from above downward, these were the umbilicus, the posterior wall of the bladder constituting exstrophy, a small penis with stunted prepuce, in the condition of epispadias, and an imperfectly formed scrotum, containing one testicle. Pushing up the tumor the orifices of the ureters could be brought into view, from which the urine incessantly dribbled, keeping the little sufferer constantly wet. This was a case of great interest and rarity. Dr. G. had seen but six or seven instances of it, one of which was figured in his work upon surgery.

He should direct the use of an apparatus which would afford protection to the irritated parts, and retain the urine in a metallic reservoir.

Dislocation of Humerus reduced.—This was of re cent occurrence, and presented all the signs of luxation into the axilla; limb elongated, elbow separated from the trunk, body inclined toward the affected side, remarkable prominence of acromion process, and flattening of deltoid muscle; these were the important leading symptoms of dislocation of the humerus. The sign, pointed out by Prof. Dugas, of Augusta, Ga., namely, inability to place the hand upon the shoulder of the sound side, would be valuable if it were not for the fact that the other signs are so decided as to make it altogether unimportant.

After enumerating the various methods of reducing this luxation, Dr. G. made an effort to reduce it by the knee in the axilla. Although aided by assistants, the muscular resistance made the effort ineffectual. Ether was then administered, the patient placed upon his back, and reduction was readily effected by the heel in the axilla; the head of the bone returning to the socket with an audible snap.

Talipes Valgus.—In this deformity there was no shortening of the tendo-Achilles, the difficulty was owing to tension and shortening of the peroneal muscles, both long and short.

These muscles were divided in this case by subcutaneous section, one-third of an inch below the fibula. A very delicate knife was introduced almost perpendicularly beside the tendon, carried beneath it on the flat side, then turned, and the division completed from within outward by a sort of sawing motion. The operation was known to be completed by an audible crackling when the tendon yielded, and by the operators being enabled to bring the foot to its natural position. The operation is almost bloodless—the apparatus for club foot was to be applied and worn.

COLLEGE OF PHYSICIANS AND SURGEONS, NEW YORK.

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Disease of the Mitral Valves.—The patient was a girl about 18 years of age, of somewhat delicate frame, but otherwise of tolerably healthy appearance.

Physical Examination —The apex of the heart strikes at a point about four inches distant from the median line—perhaps a little more than that. You see in a person of her sized frame, that this is a pretty large measurement. The heart beats quite strongly, but is regular. Here is a point that I wish to call your attention to. As I listen here in the region where you expect to hear the sound of the mitral valves most distinctly, I can detect a very loud systolic murmur; then as I come up toward the aorta, I lose it in a great measure; then as I come downward I gradually catch it again, until below the third rib the tone becomes rougher and rougher. This murmur may be produced either by the mitral or the aortic valves. If seated in the aortic valves, the sound can be followed upward for some distance into the large vessels. In regurgitation by the mitral valves the sound is transmitted downward.

This case is, then, one of hypertrophy of the heart, with regurgitation through the mitral valves. The question now arises, how did this disease originate? It is not due to rheumatism, and it has evidently existed for some time, although the disease was not named until three years ago. It may be congenital. I have known children, the first time the chest was ever listened to, to present signs exactly of this character; they have grown up with them. It does not seem that the difficulty in this case originated from any acute disease; it is the result of some slow and gradual change in the valves, probably not atheromatous. This girl is too young for atheroma, which occurs later in life. This slow and gradual change has caused some thickening of the valve, probably followed by ossific deposit, although that we know nothing about. We *do* know, however, that the valve is drawn down, and does not close well upon the blood, but allows it to regurgitate.

Now, in regard to *prognosis*; I wish to impress upon your minds, that these diseases, beginning in early life, are far less disastrous than when beginning at a later period. Persons who are growing, when troubles of this sort commence, seem to have the power of accommodating themselves to them in a degree that is quite surprising; you need not, therefore, make an unfavorable prognosis in a case of this kind. I have known a trouble of this sort to be carried about for twenty or thirty years. I could name a very considerable number of persons who have had such murmurs as these in their youth, now active business men, going on with their work like ordinary persons. So it may be with her.

Now, then, with reference to *management*, for there is no treatment. Her general health appears to be tolerably good, so there is nothing to treat her for; but a good deal that is important may be *said* to her, all of which has reference to physical exercise and controlling of the mental emotions. She must make it her business to go through life in the quietest, easiest way in the world. Never to get angry with any one. She must do everything slowly and quietly; she must not carry a pail of water,

absence of well marked physical signs in the chest, seemed opposed to that conclusion. On the whole, he had supposed it might be a cancerous disease of the peritoneum or of the pancreas. His first impression turned out to be correct.

Dr. Da. Costa, who had seen the specimens, supposed that the peritoneal disease might be the result of slow inflammation with successive layers of exudation, caused originally by a deposit of tubercles of many years back; and this was, indeed, the most reasonable explanation of the phenomena.

The pulmonic tuberculous exudation had undergone the process of cure described by Rokitansky—a conversion into obsolete tubercle, with large deposits of black pigment matter. This black pigment was very frequently found surrounding old tuberculous deposits. It occurred, also, in old people, independent of other disease, but never to the extent witnessed in this patient.

This case was also interesting and important in another view. It should make us very careful in giving an unfavorable prognosis. The physician was not to pronounce a patient doomed to death unless well assured of his facts. This patient had lived for years, apparently well, with only a portion of one lung free from tuberculous deposit.

Dr. M. related, in this connection, several instances of remarkable tenacity of life, from his own observation, where the physician might have been tempted, from appearances, to despair of recovery. He also referred to two memorable instances in the great house of Guise, one of the founders of which, the Count d'Aumale, was left for dead, with many wounds, on the field of battle, but who recovered, and whose son, then Count d'Aumale, and afterwards the first Duc de Guise, formed the subject of the famous story of old Ambrose Parè. This young nobleman was pierced through the head by the spear of an English horseman. After being pronounced hopelessly wounded by the surgeons who first saw him, old Parè extracted the spear, and the Count survived to lay the foundations of the noblest and most renowned of the princely houses of France.

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JEFFERSON MEDICAL COLLEGE, PHILADELPHIA.

SEPTEMBER 28TH.

Clinic of Dr. Gross.

Tertiary Syphilis.—This patient, a female, had presented herself on a previous occasion with two ulcers upon the leg, having hardened and excavated edges, and covered by a persistent diphtheritic slough. They had existed two years, and were surrounded now by a dark red patch of inflamed skin. An application had been made to the ulcers, of the acid nitrate of mercury diluted with two parts of water. The ulcers had put on a better appearance; they were red and granulating. She had also been put under the use of iodide of potash, ten grains, with corrosive sublimate, one-tenth of a grain. She comes back now more comfortable in every respect.

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Now, then, with reference to *management*, for there is no treatment. Her general health appears to be tolerably good, so there is nothing to treat her for; but a good deal that is important may be *said* to her, all of which has reference to physical exercise and controlling of the mental emotions. She must make it her business to go through life in the quietest, easiest way in the world. Never to get angry with any one. She must do everything slowly and quietly; she must not carry a pail of water,

a scuttle of coal; she must not go up stairs rapidly, *so as never to make the heart beat faster than is absolutely necessary for the performance of its function.* Still she wants exercise, which must be taken in a quiet way. She may walk slowly at the rate of two or three miles a day, but no more. Then she must not walk up hill, but just let the heart go on easily and quietly with all its duties, and it will give her very little difficulty.

Now, with regard to eating. She never should eat much at one time. It is better to eat four or five times a day, and to eat little, than to fill the stomach full. Repletion is decidedly objectionable. The food must be wholesome. In addition to this she must have fresh air—good nourishment and fresh air. This is about all you can say to her. Control the emotions, so that they will not make the heart beat unnaturally quick; control the physical exercise, so that it will not produce an unnecessary exertion of the heart. Take exercise, eat good wholesome food, and go through life as easy as you can, daughter. This is all I can say to you.

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Clinic of Diseases of Women and Children.

By GEORGE T. ELLIOT, M. D.

Removal of Polypus from the Uterus by the Ecraseur.—Before the patient came in, Dr. Elliot made substantially the following remarks:

I have a collection of instruments here designed for the removal of polypi, according to their position and character, in different ways. It will depend upon whether the tumor is pediculated or whether it is interstitial. A pediculated polypus may be attached to any part of the uterus. You can remove it by tearing it away, by putting a ligature around it, or you may twist or cut it away. I have even seen them removed by the application of strong caustic. Sometimes you may draw the polypus down and pull, pull until you bring it away. It is remarkable to what an extent the uterus can be pulled down in this way; but it is bad practice to pull the uterus down to such a tremendous extent as can be done. I have seen, in a case where the cervix was amputated, the uterus pulled down until the neck was almost entirely external. Under these circumstances patients have, however, been known to die from peritonitis. The mobility of the uterus gives us a means by which we can get at the polypus in question. There is a great variety of hooks and instruments which have been invented for this purpose, but they all ought to be made with an obstetric lock, so that you can introduce the blades singly, separate the hook, then lock it and draw down the uterus. If the polypus be a small one, you can very readily lay hold of it by some such instrument as this, (the common polypus forceps.) In a large number of cases you may seize it with this instrument and remove it by torsion or twisting, or you may cut it away with the instrument I show you now, the polyptome of Simpson. You see that it is probe-pointed, so that if it slips it will not injure the vaginal wall. Again, you may cut it away with a pair of scissors.

There is sometimes a little hemorrhage in this operation, but as a general rule not much. These polypi may be composed of almost any element that enters into the formation of the uterus: you may have them fibro-cellular or mucous, or dilated, containing fluid, or very vascular, from an hypertrophied condition of the mucous membrane.

Here is an instrument to carry a wire, which is designed to be passed around the polypus and drawn tightly upon its neck; then the instrument is left in situ, until the wire gradually cuts its way through. It is the original instrument of Dr. Gooch. This method I think will gradually be abandoned, because, it is very slow, decidedly disagreeable, and because we have instruments that retain all the advantages of this old form, without any of its inconveniences. Here is the instrument which I will use to day; it is the *écraseur of Chassaignac.* With this instrument, if you can succeed in getting it around the stem of the polypus, all you have to do is to steadily and slowly work the handle until the polypus comes away. The advantage of this instrument is, that there is very little hemorrhage, indeed, after the operation.

The patient was then brought in. She is thirty-two years of age, has had two children, the last six years ago, and presented the general symptoms of uterine polypus.

The polypus which we have in this patient is, so far as I am able to ascertain, attached to the cervix. It is small, and you perceive she has not yet suffered from the hemorrhages to which it would most certainly give rise were it allowed to remain. It is one of those fortunate cases in which you can save the patient from a great amount of suffering.

The speculum of Sims was then introduced, the patient placed on her hands and knees, and the chain of the écraseur being readily brought around the neck of the polypus, the latter was removed.

In this case the écraseur, which I have used for the first time, *has proved itself to be very efficient.* I had no difficulty in passing it into the uterus, placing the chain around the neck of the tumor, and I entertain no doubt that some such process as this will entirely supercede the ligature. If you have a very large polypus, you can leave the écraseur around the tumor for hours, and give the handle a turn once every ten minutes until the tumor is removed.

Medical Societies.

IMPORTANT DISCUSSIONS ON

YELLOW FEVER AND QUARANTINE.

Before Medical Societies in Philadelphia and New York.

PHILADELPHIA COUNTY MEDICAL SOCIETY.

[Reported by W. B. Atkinson, M. D.]

Regular Meeting, Sept. 14, 1859.

Dr. Jewell read the following paper:

"*Has yellow fever ever originated in the latitude of Philadelphia?*" is the deeply interesting and highly important question suggested by the Committee on Lectures, and the subject for our investigation on this, the opening occasion of our winter's campaign of conversational meetings.

The question under consideration conveys, in its meaning, two propositions, viz: the domestic, and the foreign or exotic, origin of yellow fever—one of the most fearful and fatal of all epidemic diseases.

Dr. La Roche, in his elaborate and distinguished work on yellow fever, has presented to the wide world the name of Dr. W. Jewell, (your humble speaker,) as an importationist; while on the other hand, Dr. J. C. Simonds, of New Orleans,—in the report of the Sanitary Commission of that city, alluding to the same documentary evidence as referred to by the former gentleman—says that I am an advocate for the domestic origin of yellow fever. I introduce these two antagonistic opinions in order to avoid the implication of inconsistency, in the event of my embracing either the one or the other horn of the dilemma in which I find myself placed by these eminent writers.

If facts make up the history of any subject under investigation, it becomes me in the present instance to take simple facts, unbiassed by favorite theories or plausible pleadings, and place them impartially before you, in order to receive any light upon the origin and development of yellow fever.

To narrow down my introductory remarks to a single point, and desirous, also, to relieve you from any anxiety or doubt as to the opinion I entertain of the question before you, I will premise the few words I shall say, by giving you to understand, that I have no hesitation in believing that yellow fever not only may, but has originated in the latitude of Philadelphia, entirely independent of all modifying influences, and all labored efforts to connect its origin and existence with a foreign poison. At the same time, and with the same candor, I desire to be distinctly understood as acquiescing in a prevalent sentiment, that yellow fever may be, and has been introduced into the latitude of Philadelphia through foul vessels.

In proof of the assertion of its domestic origin, I will only carry you back to 1856, and remind you of the cases of yellow fever, five in number, that occurred in our own city. Being local or sporadic in their character, and not having been followed by a desolating train of consequences, no one that I know of ever attempted to entangle them with any foreign alliance. The same may be told of the cases of 1854. Nor should I omit to include, on the authority of Dr. La Roche, those of 1801 and 1826, wherein the most persevering contagionists could not find the smallest chance of deriving the disease from abroad. It is unnecessary to multiply authorities and cases. I have confined my proof to our own city, and even here, I might, with propriety, enumerate other analogous occurrences of the disease; but I forbear. I will only, however, allude to the fact, that Dr. La Roche, in his work on fever, and Barton, in his sanitary commission of New Orleans, offer such abundant evidence of the local or domestic origin of yellow fever, not only in tropical, but in temperate locations, as to stamp the opinion with the seal of truth.

Again, and here I quote the language of Dr. La Roche, "If sporadic attacks of the yellow fever are admitted to be the result of domestic causes, and to arise independently of importation and contagion, it is difficult to understand on what grounds the disease is denied to be due to the same causes, and why we should attribute it to a foreign source, in those instances where, instead of appearing to a limited extent, it spreads over a broader surface, and, assuming the garb of an epidemic, attacks a large number of individuals. It cannot be hazarding too much to say, that in both instances the cause must be the same." Now, I infer, that in the former instance there is no prevalent epidemic constitution of the atmosphere to favor the virulence and spread of the disease, while in the latter, the peculiarity of the hygrometrical, electrical and thermometrical states of the atmosphere favor to a far greater extent the evolution of the febrile poison, and gives to it greater energy and diffusion.

The remaining proposition in the question looks to the importation of yellow fever from abroad. It would seem hardly fair to discuss the one, without an allusion to the other. Indeed, it is almost impossible to separate them, in my own mind; for while I admit a domestic origin for the disease, I am unwilling to lose sight of the fact that yellow fever has been conveyed by foul vessels into places and ports from tropical climates, where the disease occurs endemically. This fact is acknowledged by the most strenuous advocates for the indigenous origin of the disease, and that, too, in the latitude of Philadelphia. Even the distinguished Dr. La Roche receives this opinion to a limited extent, for he says, (I quote from vol. 2, p. 582,) "It must be borne in mind, in addition, that in every instance in which the disease has been traced to a ship, its cause was local in the latter, and, as already stated, did not

spread its morbid influence beyond a short distance, and that the influence ceased to be felt after its removal, or the dissipation of the effluvia it had left behind;" and in the 1st vol. of his inimitable work, at p. 593, he further says, "the yellow fever has often been known to arise from the foul exhalations of ships." Now, as regards the infection of a ship, arising invariably from foul bilge water or the decomposition of vegetable and animal materials under the limbers or in the seams of the vessel, the inference is not altogether clear to my mind. During the present season a ketch from Havanna was detained at our Lazaretto, having lost several of her crew with yellow fever; but her condition was by no means foul, and yet, during the time of discharging her cargo, both the physician at the station and the inspector of vessels contracted the fever, the latter of whom died. Indeed, it would not be difficult, did time allow, to bring forward numerous instances of infected vessels arriving at our ports from the tropics, where they had cases of yellow fever originating on board from port to port.

But allow me to dwell for a moment upon those cases where the fever has originated from a local cause on board of a ship. This infected vessel arrives in a healthy port, then, according to the evidence of Dr. La Roche, and with which I entirely agree, its morbid influence may spread, not beyond a short distance, he says, and the effluvia may not be entirely dissipated after its removal. This concession on the part of that distinguished author affords me a foothold on which to build my opinion that yellow fever may be, and has been imported into the latitude of Philadelphia. And I go further, and believe that this yellow fever germ or miasm, or whatever else you please to designate it, originating either from putrescent materials on board the vessel, or confined in fomites, or remaining in the confined atmosphere of the hold during the voyage—if introduced into a contaminated atmosphere, and thus finding materials to feed upon, and capable of exhaling a morbid effluvia—it serves as the nucleus or ferment, and in an atmosphere with an epidemic constitution, the result will be the prevalence of a disease, the extent and malignity of which will depend upon the condition of the poison eliminated.

Heat and moisture are generally acknowledged to be the two active agents in the production of the poison; and wherever, either within the tropics, or in temperate climes, or on board ships at sea, we have this combination existing in the midst of surrounding infusoria favorable to fever of a malignant type, there we may have yellow fever. What the exact condition of this state of chemical action must be, in order for the development of the disease, no one has attempted successfully to define.

I therefore entertain the opinion that there exists, at certain periods and under certain meteorological and terrene conditions, an epidemic constitution of the atmosphere, which is susceptible of the production of a poison peculiar to the development of yellow fever in the human system. This poison, I hold, is capable of reproduction, or will prevail in locations badly ventilated and fruitful in filth, where there exists a crowded and vagrant population, to a far greater extent than in more sparsely settled, better ventilated and more cleanly neighborhoods.

Therefore it is a matter of indifference to me where this poison is produced, whether on ship board, in a foreign port, or on shore in our own city, provided the meteorological and terrene phenomena be such as I have described, we shall be liable to have the fever break out and spread to a greater or less extent, according to the influences at work and the local peculiarities of the surrounding neighborhood.

Who will deny that the few cases of yellow fever which were found in our own city last year, (1858) were the offspring of a poisoned atmosphere in and about the two West India vessels that had escaped quarantine, and lay moored in the vicinity of Lombard street wharf? These vessels had arrived here in the month of July, from ports where yellow fever was prevailing at the time of their departure, and they had lost several of their crew from the disease on the voyage home. A custom house officer who had one of the vessels in charge, and who resided at Roxborough, went home sick, and died of yellow fever on the 5th day after his attack. A night inspector of the same vessel also took the fever from the infected atmosphere on board, and died on the 6th day. Besides these two cases of direct communication with the vessels, nearly all the others could be traced directly to a residence or to a visit in the immediate vicinity of where they lay.

But let me refer you to another instance of similar character of less recent date, and which, from the peculiar circumstances accompanying the results, has been made the subject of much discussion among the advocates for the domestic and foreign theories, wherever its history has become known. You will anticipate me, that I have in view the case of the *Mandarin*, and the yellow fever in this city in 1853. Of this epidemic the most of you were eye-witnesses: I need not, therefore, occupy your time with the details. My principal motive in calling attention to it here, is to repeat, what I have elsewhere asserted, that this vessel, in my opinion, was instrumental in originating or introducing the infection into our city, by means of a morbid effluvia generated under her limber planks, from a putrescent state of her bilge-water; and notwithstanding all the arguments—and they carry with them great

weight—that the able Dr. La Roche has brought forward to refute the position I then held as to the origin of the fever, I am in no wise shaken in the sentiment. If the barque Mandarin had no hand in infecting the atmosphere in the vicinity of South street wharf, in 1853, then am I persuaded that all writers advocating the local origin of yellow fever on board of foul ships have erred; that even Dr. La Roche, with all his deep researches into the nature and origin of yellow fever—and no man living has waded more thoroughly through the history of the disease, or posesses greater facilities for obtaining information and knowledge in his own extensive and unrivalled library of works on yellow fever, containing, with but few exceptions, everything that has ever been written on the subject, "from the tiny pamphlet to the fat and stately quarto, or even folio"—even Dr. La Roche himself has erred; and that Dr. Harris, in his account of the spread of the yellow fever at Fort Hamilton, on the Long Island shore, Brooklyn, Governor's Island, Staten Island and New Jersey shore, ascribing the same to the spread of an infected atmosphere in and surrounding the yellow fever fleet at quarantine, during the summer of 1856, has also erred.

I am aware that Dr. La Roche has cited an isolated case of yellow or malignant fever which is said to have occurred in the city ten days or more previous to the arrival of the Mandarin—though at the distance of two-thirds of a mile from South street ferry—although it was not certified to as yellow fever at the time, and that he lays much stress upon the fact that none of the stevedore's gang or other persons engaged in unloading the Mandarin, nor that any of her sailors, took the disease. All this may be admitted, and still the argument I have advanced is not weakened. The isolated case referred to rather strengthens my opinion, as it goes to prove the existence of a suitable condition of the atmosphere to elaborate the poison under propitious circumstances. And as to his argument of the escape of the stevedores, laborers and crew of the Mandarin, it proves nothing more than that they, like hundreds who were similarly exposed to the infectious atmosphere in the vicinity of South street wharf, were also inocuous to the poison. As well might we use the argument, that because all the boarders and the family at the Champion House, at South street wharf, were not stricken down with the disease, hence it could not have been yellow fever.

I have not time allowed me, nor would it be pertinent to the occasion, to engage in an examination of all the points taken up and argued by Dr. La Roche, in his endeavors to disprove the agency of the Mandarin in introducing the fever of 1853 into our city. Suffice it to say that the facts bearing on her instrumentality in the case in question are so clear to my own mind, as to defy the ingenuity and philosophy of the most erudite to obscure them.

I have thus, Mr. President, taken a very brief and hasty view of the question offered for the evening's discussion. The subject is one of interest. As physicians of a great commercial city, it becomes us to familiarize ourselves with its history, its nature and origin, its sporadic and epidemic visitations, its pathology and treatment. There is enough, however, in that department of the subject before us by appointment this evening to engage our undivided attention, and I trust the members present are not only prepared, but will come up spiritedly and cheerfully with the numerous arguments which may be advanced for and in behalf of either side of the debate. I have opened the way, I expect others to follow.

Dr. CONDIE remarked, that from the manner in which the question proposed for discussion this evening was worded, he had been undecided whether the inquiry was as to the origination of yellow fever in places situated upon the same parallel of latitude as Philadelphia, or simply, has it ever originated in this city. The two questions are very distinct from each other, and open up somewhat different fields of investigation. Finding that the gentleman who has just sat down confined his remarks to the origin or non-origin of the disease in Philadelphia, Dr. C. would in those he was about to offer, follow his example.

That yellow fever has repeatedly occurred in this city, and in some of its occurrences here, has prevailed somewhat extensively, will be admitted, he presumed, by every one. Now, we know of only five ways in which its occurrence can have taken place.

1st. From endemic causes. That is, from strictly local morbific conditions,—aerial or meteorological, telluric, and accidental.

2d. From epidemic causes. That is, from some general morbific condition of the atmosphere, which, as simply a generator of disease, would seem to act in some degree altogether irrespective of local appreciable conditions, whether of the atmosphere or of the soil, or connected with the civic and domestic relations and industrial pursuits of the inhabitants.

3d. From contagion, brought from abroad in the persons of those laboring under the disease, or in their clothing or baggage; or in certain portions of the cargo of a vessel recently arrived from an infected port.

4th. From a zymotic germ,—a morbific ferment, imported in the atmosphere of a vessel, in the clothing of its crew or passengers, or in its cargo, from a place at which yellow fever prevailed at the period of her departure.

5th. From an especial morbific condition of the

atmosphere of a vessel, and of her cargo, arriving at a port, after a long voyage, at a particular season of the year, and under, perhaps, special meteorological conditions; whether the vessel had come from a yellow fever port, or had cases of the disease on board, or not.

We can conceive of no other manner save one or other of the foregoing, by which the yellow fever can be produced in this or in any other city.

It is to be remarked that when the disease has occurred in Philadelphia, it has always been at a particular season of the year, and one marked by certain peculiarities of atmospheric temperature and moisture, and always in certain circumscribed localities. It has never spread generally, over a large portion of the city, or throughout its entire limits.

To examine carefully into all the facts connected with the occurrence of yellow fever, at each of its visitations to this city, with the view to a settlement of the vexed question as to its domestic or foreign origin, would occupy too much time. Neither could it be done without the preparation beforehand of a docket of the leading facts in each case, with their particular relations and bearing. In our attempt, to-night, to solve the question proposed for discussion, we must adopt some more comprehensive, and under the circumstances, feasible plan. The best probably is that by exclusion. Let us endeavor to discover by which of the already referred to modes of development, the occurrence of yellow fever in Philadelphia cannot, with a due regard to facts, be accounted for, and we may, perchance, be led to the discovery of the true source of the disease, when it makes its appearance in our midst.

In the first place, the occurrence of yellow fever in this city cannot be referred to an imported contagion,—whether conveyed to us in the persons of the sick or in fomites of any conceivable kind. There appears to us to be no question connected with the etiology of any disease more fully settled than is the non-contagious character of yellow fever.

The simple fact that the disease is confined to particular seasons; that it prevails only under particular meteorological conditions, and within certain well circumscribed localities, characterized by bad hygienic conditions, is strong prima faciæ evidence of its non-contagious character. Of which of the confessedly contagious diseases can the fact just referred to be, in truth, predicated?

The arrest of the disease by removing all the inhabitants from the infected districts, and admitting no person subsequently to enter them, until after the occurrence of frost, is another fact which disproves its contagious character; and still another is the free admission of yellow fever patients into the sick-wards of our public hospitals, and their treatment there in the immediate vicinity of patients affected with other diseases, without, in a single instance, the spread of the fever to the latter, or to the nurses and medical attendants.

The fact that yellow fever most generally has made its appearance in Philadelphia, upon one or other of the Delaware wharves, in the immediate vicinity of the foreign shipping, is adduced as a strong evidence that the disease is the result of an imported contagion. But we are to recollect that in a season of sufficient heat, the wharves present all the circumstances favorable for the generation of a malarious atmosphere adapted to produce the most malignant forms of yellow fever. Our wharves are badly constructed, out of perishable timber, and filled up with a porous mass of decomposable and partly decomposed material, which is kept constantly soaked by the water of the river. The surface of the wharves and the heads of the numerous docks receive the filth and various refuse matters from ships loading and unloading, from a number of coasting and market boats, and from the neighboring stores and dwellings. Into the river, at the wharves, open the mouths of all the sewers which drain the most populous portions of the city. Much of the filth thus brought into the river, accumulates in the docks, and at low tide lies exposed to the sun, a fruitful source of the most pernicious malaria. The wharves, too, are invariably, in some places, many feet below the level of the part of the city in their immediate vicinity; they are bordered by large warehouses for various kinds of merchandise and produce, or by high dwellings, which, with the narrow courts and lanes that occur along much of the Delaware river front of our city, are inhabited each by a number of families, by whom little attention is paid to the requirements of personal or domestic hygiene. The malaria constantly generating from all the foregoing sources, along our wharves, and the humidity arising from the neighborhood of the river, have little chance of being dissipated, where everything in the neighborhood thus conspires to prevent a free ventilation. Hence, in seasons of intense heat, our wharves must necessarily become the hot-beds of disease.

Yellow fever cannot be said to have ever occurred in this city as an epidemic, in the strict sense of the term. Its appearance has always been surrounded by too many specialities of season, meteorological phenomena and local conditions, to be considered in the light of an epidemic. No epidemic, that we know of, breaks out in certain narrow circumscribed localities, beyond the limits of which it rarely spreads. Such a disease is an endemic of a particular place, originating in strictly local causes, and has not any affinity with an epidemic, dependant upon some widely spreading distemperature of the

atmosphere. Small-pox, cholera, and influenza, if they do not occur simultaneously over large cities or tracts of country, progress invariably from one point to anotner; from east to west, from north to south, or vice versa; along a wide or narrow strip of the earth's surface, in a comparatively short space of time. It is very certain that certain morbific conditions, existing at particular points, often invite, as it were, epidemic maladies to prevail there to a greater extent, for a longer period, and with more intenseness, than in healthy localities; nevertheless, such local morbific conditions are by no means necessary, as they are in the case of yellow fever, for the occurrence and prevalence of an epidemic disease.

The hypothesis, first advanced by Dr. Hosack, of New York, but essentially altered of late years in some of its details, which assumes that yellow fever, or rather a yellow fever producing atmosphere, may be generated in any eminently malarious locality, during the heat of summer, by the introduction of a zymotic germ through the air, or cargo of a vessel arriving from a yellow fever port, requires only a passing notice. We admit that this hypothesis presents an etiological position which may or may not be true—that there is nothing impossible about it. Its adoption gets us rid of many difficulties with which both the theory of the domestic origin of yellow fever, and that of its importation through a contagious matter in the clothing or persons of the passengers and crews of vessels arriving from infected localities are encompassed. It unites upon the question of importation the advocates of the contagious character of yellow fever with the non-contagionists—the advocates of the strictest system of quarantine with those who would mainly depend for the prevention of yellow fever upon the enforcement of a thorough and enlightened system of public and domestic hygiene. Unfortunately, however, this very convenient theory of atmospherical zymosis by the introduction of a foreign morbific germ is merely a plausible hypothesis, unsupported by a single well established fact or analogy. It is, after all, only the revival, under a modified form, of the repudiated doctrine of contagion as applied to yellow fever. As a most convincing evidence of the truth of this theory, its advocates assert that yellow fever always makes its appearance, when it occurs in our midst, subsequently to the arrival of infected vessels, persons, baggage, clothing, merchandise, etc.; with the latter, therefore, and the occurrence of the disease, it is inferred that there is an intimate connection—by them, it is argued, a poison is introduced which, under certain favorable circumstances, has the power so to change the condition of the atmosphere of the place as to render it a yellow fever producing atmosphere. But neither the fact thus asserted, nor the conclusions that have been drawn from it are founded in truth. It is not true that the appearance of yellow fever in this city has been, in every instance preceded by the arrival of infected vessels, persons, clothing, baggage, etc. Such was not the case, certainly, in 1805 and 1819. Even when the outbreak of the disease has followed promptly the arrival of a vessel from an infected locality, it has not always been proved that any infection existed on board of her, or, when such infection did exist, could any connection be traced between the vessel and the cases of fever which occurred among our own citizens. In some instances, it would seem more than probable that the very vessel which had been accused of bringing the disease to our shores, has actually had it produced on board of her while she lay at one of our own wharves, from the same local causes which produced it in such as dwelt in or visited the infected district in which she had her berth.

Yellow fever has frequently made its appearance in mid-ocean, among the company of a ship that had sailed from a perfectly healthy port. In such cases from whence was derived the zymotic germ, or the contagion? Equally difficult will it be found to explain, by the zymotic theory or by that of contagion, why it is that the yellow fever has attacked on board of a foul vessel undergoing repairs, those only who worked in the lowest part of her hold, while, all the workmen without a single exception, who were engaged in other parts of the vessel escaped. Both theories are incompatible with the well-known fact that the infected air productive of yellow fever is often strictly confined within the limits of certain well-defined localities, and that by removing all persons from such infected locality, and preventing any one from entering it until after the setting in of frost, yellow fever may be arrested by preventing most effectually the possibility of the occurrence of new cases.

We believe that the foul air of a ship's hold will, under certain circumstances, after her arrival in port and the opening of her hatches, produce the yellow fever in her crew and such persons as come on board of her, as well as in those who are in any way exposed to the pestilential air emanating from her; nay, further, we believe that there is overwhelming evidence to prove that when a large number of foul vessels are crowded together in a damp, hot atmosphere, the pestilential air generated on board of them may be wafted by the wind to perfectly healthy localities in their immediate vicinity, and in so undiluted a state as to produce there, disease. Now, it is to be borne in mind, that "the importation" of yellow fever in this manner, may be, by vessels coming from ports where no yellow fever—no disease, indeed, of any kind prevailed at the

period of her departure, nor immediately preceding or subsequent thereto, and without having on board, previously to her arrival, a single case of yellow fever or of any other disease. It must be recollected that the vessels referred to, with their foul atmosphere constitute to all intents and purposes infected districts; by those who reside or come within which only can disease be contracted, and with such the disease stops—it never becomes endemic to any other locality than the ship's hold. It cannot be shown, that in a single instance they left behind them an infected atmosphere capable of producing yellow fever.

We have only one set of causes by which we can account for the occurrence of yellow fever in our own city, and that set are strictly endemic, they occur in our very midst, and to a very great extent are under our control—for although in the production of yellow fever there no doubt enters some other element than atmospherical heat and moisture, and an accumulation of putrescent or decomposible materials, and notwithstanding our entire ignorance of the nature of that other essential morbific element, still, it is very certain that it is of itself inefficient for the production of disease, and that by removing or guarding against the other morbific agents which give to it efficiency, the occurrence of yellow fever may be prevented, or if the disease do occur it may be circumscribed in ex tent, and disarmed of its malignancy. From the foregoing considerations we are convinced that the proper answer to the question under discussion is in the affirmative—yellow fever has originated in Philadelphia.

DR. DARRACH asked the use of the present form of quarantine, if yellow fever be of home origin?

In 1853, and subsequently, several unqualified cases of this too fatal disease were under his observation. He witnessed also several autopsies, some of which were made by himself, and that with much accuracy and the deepest interest. In regard to the clinic of said cases, its category of symptoms was peculiar—entirely different from that of malignant remittent. It consisted of inaction of mental faculties without delirium, emotionless, torpid functions, especially that of the liver, and extreme depression of strength. As in cholera, so here the breath and yellow skin were inodorous and of low temperature.

In the autopsy, neither odor nor halitus were detected from the exposed serous surfaces of the chest and abdomen. The mucous surface of the prima via and the internal surface of the arteries were extensively reddened from an indelible blood-stain, which doubtless was owing to a moribund capillary exosmose. There was no extra-venous nor extra-capillary congestion.

The organ most remarkable was the *liver*. It was firm, resilient and of diminished size: and its color was that peculiar tint of yellow which in autumn is on the sycamore leaf when it passes from its brighter yellow into brown. Yet more remarkable and peculiar were its internal conditions. According to the microscopical observation of the resident physicians of the Pennsylvania Hospital, of 1853 and '54, the liver cells were filled with oil; this was also outside the cells, and these were distinguishable from the oily liver of the consumptive and the drunkard, by their disposition to disintegration. This tendency was so great that the cells under the microscope presented, in the place of the normal clear outline, a ragged edge. This yellow-fever liver is in glaring contrast with the enlarged, engorged, softened and dark liver of malignant remittent, especially that which Johnson describes in the Hoogly case. This implies a veno-hepatic congestion and inflammatory softening, whilst that of uncatenated yellow fever might seem to signify dehepatization with palsy of the capillary.

He regarded yellow fever, therefore, as a peculiar disease, non-contagious and of tropical origin.

He was aware that often at Philadelphia the month of July had a tropical character. But vegetation here is not then under decomposition; we have never the rank vegetation of the tropics, and it has not been in this month that the fever in question has here prevailed. Besides all this, the question arises if vegetable decomposition can produce more than the miasm of inter- and remittent. The etiology of yellow fever is somehow associated with the tropical vegetation in connection with the oceanic salts and alkaloids, and the filth of neglected sea vessels. This triple compound under the zenith rays of a tropical sun may produce the poison which being incorporated into woolen and cotton goods, and other substances may be exported from the tropics where it was fabricated. If so, then exclude it, and if it has entered, fence off the infected locality, and properly empty it of humanity. He did not fear the removal of the cases from the infected spot when washed and re-clothed. They could not communicate the disease. He illustrated this idea by the supposition of a spot infested with serpents where, if a person goes he receives a mortal wound, but cannot communicate it.

In respect to the specific nature of yellow fever, it may consistently, like intermittent and remittent fever, and scarlatina and other epidemics, have its scale of virulence and its acme and decline, and thereby assume different modes.

DR. BELL said that the question, as propounded, might seem to imply a doubt whether the yellow fever ever prevailed in Philadelphia. The subject, if taken up as a matter of original inquiry, may be examined in two ways—first, by *à priori* reasoning;

second, by the aid of history. Knowing the nature of the climate, the season, and the particular localities in which yellow fever finds its origin, and a congenial home, as in the West Indies and the maritime region surrounding the Gulf of Mexico, as well as those spots in southern Europe in which it occasionally occurs, we have but to compare those regions and places with that portion of Philadelphia along the Delaware river, and then ask whether, in the tropical heat of many of our summers, the exposure of a part of the bed of the river at receding tides, and the mud and vegeto-animal matters, of which it largely consists, acted on by a fervid sun, similar matters poured into the stream from numerous sewers, and sometimes alike exposed to high solar heat, not to speak of the made ground of our wharves and the decayed wood of their facings, are not adequate to the production of yellow fever, certain atmospheric, but not well defined conditions, added to high heat, favoring? The climate, as far as relates to the tropical heat of our summers, topography and the occasional adventitious circumstances of faulty public hygiene in Philadelphia, are analogous to those which are constantly found in the admitted homes of yellow fever. But—and herein is a difficulty in the way of etiology not yet solved—in all that portion of the eastern hemisphere which embraces Asia and its islands, there are extensive districts with maritime exposures and alluvial soil, at the mouths and on the margin of rivers, in which, so far as regards climate and all the atmospheric and terrene state of things, we see all the conditions for the generation of yellow fever, and yet this disease is never met with. Why, for example, should not yellow fever appear as regularly at Calcutta and the Delta of the Ganges as it does at New Orleans and the lower country of the Mississippi?

The historical side of the inquiry exhibits little that is satisfactory. You know what was said of Herodotus, the father of history, that he was the father of lies—an unjust stigma, but I only refer to it now to remark on the extreme difficulty, even when the documents are collected, of writing a history which shall truly represent the facts and incidents as they really occurred. In the question before us, respecting the alleged foreign origin of the yellow fever, we are almost entirely wanting in the first series of facts, viz: the actual sanitary state of the port at the time of the departure of the vessel which was represented to have brought with it the *materies morbi* in the shape of a virus, communicable by contagion, or a miasm, transmitted by contact of the sick with the well. Now, whether we regard the defective evidence in the first part of the inquiry, or the failure of proof in the second or home branch, we can confidently say that a case involving questions of property or of life not better sustained by more connected testimony, would inevitably be thrown out by a jury as "not proven." The commonest mistake in arguing is, that lapse of logic, which assumes two coincident facts or phenomena as bearing to each other the relation of cause and effect. In medicine our progress to a philosophical system is continually embarrassed in this way; it confuses our etiological explanations; it misleads in our therapeutical deductions and clinical experience. It is by a fallacy of this kind that a vessel arriving here from a suspected or diseased port, contemporaneously with the appearance of the yellow fever, is assumed to have imported the disease. Often, in cases where such a cause has been assigned, it has been shown that the outbreak of the fever had preceded the arrival of the accused vessel, and also, that cases of the disease had occurred in spots at a distance from its alleged focus, and among persons who had not either been near the vessel or had held any intercourse whatever with the crew or passengers or any other party visiting it or working on board.

Dr. Jewell, in the interesting remarks with which he has introduced the subject this evening, takes a kind of middle ground between the advocates of domestic origin of yellow fever, and of its importation or introduction from abroad. While admitting that the former does occur, he believes also that a vessel from a port in which the disease prevailed at the time of her departure, contained material in her hold and in her timbers which, acted on by heat, evolves the poisonous miasm of yellow fever. This may be considered as a conceded point, and it has been proven by, among others, Dr. La Roche, in his great work on yellow fever. Dr. Jewell goes a step further, and in this also he is sustained by abundant testimony, viz: that the miasm thus evolved becomes a cause of disease and death not only to individuals among her crew and passengers, but also, when she arrives here, (in port,) will give rise to the fever, both among those who come aboard of her to discharge cargo, and to those other persons who are near her, but yet on land. The vessel in this case may be said to represent a state of things ashore which is known to give rise to the fever—heat, moisture, and fermenting substances in a peculiar state of aggregation But Dr. Jewell is disposed to enlarge the range of diffusion and morbid operation of the miasm evolved from a newly arrived and sickly vessel. He believes that this miasm, instead of losing its deleterious and toxica power by mixing with the air, acts as a ferment, and thus becoming diffused over a larger surface, gives rise to disease on a more extensive scale. He admits a certain predisposition in the state of the air to be thus affected by the miasm, but he is unable to define in what it consists; analogy fails to aid him.

Were he to compare the process to cremacausis, the main condition determining which, viz. the presence of decaying matter, is wanting in the circum-ambient air, which, so far from submitting to its being converted into fresh miasm or poison, is itself, by its diluting powers, a weakener, if not direct destroyer of such poison. Still more, the combination of this miasm with the oxygen of the air would be attended, as in the case of vegetable structures, with the disengagement of heat, and occasionally of light also, a result totally different from that supposed by Dr. Jewell. Looking to what takes place on land during the existence of a yellow fever, and to the limitation of the alleged miasm extricated from the surface, so that we can trace its boundaries, and at times fence it in, we do not find any proof of diffusion of the miasm from its original source, or its increasing itself in the atmosphere in the manner supposed by Dr. Jewell.

In all our views of the etiology of yellow fever, there is still wanting an essential element, the *causa sine qua non*, to complete and give effect to the other conditions for the production of the disease—high heat and moisture acting on vegeto-animal matters. The alleged product of these known appreciable agents, to which may be added electricity, we call, hypothetically, a miasm, or volatile poison, but of its actual presence, to say nothing of its inherent properties, neither chemistry nor microscopy has furnished any proof.

Dr Nebinger regarded the question under consideration as one of great importance, deriving its value from the results which may grow out of the manner in which it may, by physicians and public authorities, be determined. If it shall be the general conclusion that yellow fever with us can have only a foreign origin, that it can never spring up from nor be aggravated by domestic causes, then will all our means of prevention be directed to controlling its introduction from abroad. If, on the other hand, it shall be determined that it cannot be introduced from foreign sources, then there must follow an abrogation of all those means now thought important in preventing its introduction from such sources. But if it shall be the wise and true conclusion that yellow fever can have a foreign origin, and be aggravated by domestic causes, then our means of prevention must be such as shall meet the double necessity of the case. We must adopt the most stringent quarantine measures, and the most thorough domestic sanitary regulations. He felt confident that yellow fever could have a foreign origin, and that its spread, continuation and malignancy could be vastly influenced by local causes. He had not arrived at this conclusion hastily. It was based upon facts and observations. It appeared to him that yellow fever was, in some of its habits, and particularly that of love of locality, like most epidemic non-contagious diseases. It flourishes most where filth most exists, where the atmosphere is most contaminated by noxious gases generated by the rapid decomposition of animal and vegetable matter mixed with earthy matter. We discourse of cholera, yellow fever, and other disease generating poisons, but what is their shape, or how or in what form they exist in the atmosphere, no man knows. All we do know is the ultimate fact that there is a condition of atmosphere where an epidemic prevails, which excites in the living body certain conditions which we call cholera, yellow fever, typhoid fever, or otherwise, according to the morbid phenomena developed. In what these several diseases generating poisons differ, it has not been the happy province of any man to make manifest. Whilst, however, the nature and form of the materies morbi are not known, there are certain facts well understood in regard to the increase of that disease generating matter, or if the word increase was objected to, then he would say there are certain well understood and duly recognized conditions which have an influence in extending, continuing and increasing the malignancy of epidemic diseases; and among these well understood conditions there is none more universally recognized than the presence of animal and vegetable matter mixed with earthy matter, and influenced by certain thermal and hygrometric conditions. How these conditions influence the spread and increase the malignancy of epidemics, is a grave question, and worthy of the most profound consideration. The bare fact of the conditions referred to having the influence stated, is generally admitted, but the how and the why it is so, is not so generally agreed upon.

The doctrine, he thought, is, that, for example, when cholera has prevailed epidemically here, that its origin has been foreign; that the cholera materies morbi has been wafted, upon the wings of the wind, from abroad. Such admitted, why, in the same city, has its ravages been much, very much more in some localities than others; and those always the foul and filthy portions of the city? Has an epidemic poison the power of self-multiplication? If so, from what material does it multiply itself? does it find in a pure atmosphere the constituents necessary for its development and multiplication? or are they only found in an impure atmosphere, rendered so by the putrefaction of animal and vegetable matter in the presence of earthy matter. From the fact, that those who reside in localities where such an impure atmosphere exists, are most influenced by the epidemic poison, the inference is reasonable, that there is something in the impure atmosphere, which, being acted upon by the epidemic materies morbi, is converted by it into matter like unto itself, and thus the epidemic poison, mul-

tiplying itself, increases in quantity, day by day, until the atmosphere of the foul locality becomes as deadly and as destructive as the atmosphere about the Upas. An almost infinitesimal quantity of the saliva of a rabid animal introduced into the blood of a healthy dog, produces, in that animal, hydrophobia, and the dog has generated, by the action of that infinitesimal quantity of materies morbi, an amount of the poison which so vastly exceeds that which was introduced into his blood, as to bear to it the relation of a million to one. The fact is conceded, that the small quantity of hydrophobic virus placed in the dog's blood acted upon the elements found there, and multiplied itself to such an extent as to develope in the dog a train of symptoms and phenomena identical with those in the animal from which it was taken. How was this done? Who will answer? May not certain disease-generating poisons, acting upon elements which they find in a vitiated atmosphere, have the power to multiply themselves out of those elements, as the rabies poison has the power to multiply itself out of elements in the blood. Starch, gum, and sugar are composed of C. H. O. in precisely the same quantities; yet how dissimilar are these three substances. By the action of some law, by some peculiar arrangement, a grouping of these same elements, we have produced three bodies bearing not the least physical resemblance to each other. May not the materies morbi of an epidemic be so controlled, by some chemical law, which enables it to act upon elements which it finds in a foul atmosphere, of the character to which he had referred, and by its action so group and arrange these elements, as to produce its kind, and thus multiply itself, until the foul locality be rendered remarkable because of the vast and extraordinary mortality produced by the epidemic? Matter changes the production of new bodies—equally as difficult to produce as the materies morbi of an epidemic in the manner stated, every moment of the day, are being induced; yet we cannot state or explain how they are produced. By the presence—that is the expression used qy chemists—of a nitrogenized body in a mixture of starch and water, alcohol is developed out of the elements of the starch. This is a purely chemical process, and when the time shall arrive for explaining the why and the how alcohol is generated under such circumstances, then, and not till then, will it do to scout the hypothesis I am pleading for in regard to the presence of the materies morbi of an epidemic being able to develope its kind, or multiply itself out of matter which it finds in an atmosphere rendered foul by the putrefaction of vegetable and animal substances, mixed with earthy material.

In 1849, when cholera in an epidemic form prevailed in Philadelphia, there was, in the southern section of the city, one locality where its ravages were so great as to render the place specially attractive—Reed street, from Front street to the Delaware river, was the locality. The mortality there was vastly greater than among the same number of citizens in any other portion of our city. So many were the victims, that it was the remark, at the time, that the undertaker's hearse was to be seen in Reed street at every hour of the day, bearing away those who had perished by the malignant breath of cholera. Why was the mortality in Reed street so much greater than the mortality among the same number of persons in other portions of the city? Let the facts be answered: The drainage of Reed street, from Front street to the Delaware, was extremely defective; the street was not paved; from curb to curb was one vast bed of black mud, enriched with kitchen refuse of several years' gradual deposit. At the end of the street, at the door of the last house, the culvert emptied thousands of gallons, per diem, of putrescent matter on the marsh, several hundred yards above low water mark. There, spread over the marsh in the most favorable manner to promote its speedy decomposition, these sewerage ejectments had generated from them malaria, which was wafted over Reed street, and which, when combined with the noxious gases generated in the street, rendered the atmosphere of the neighborhood foul, indeed. A few years rolled by, and Reed street was paved; the culvert extended to low water mark; the broad marsh, for hundreds of yards in front of, and for hundreds of yards above and below Reed street, was wharfed in, filled up, and completely reclaimed. The drainage was thus vastly improved. Cholera again presented itself in 1854. How different the scene then from the saddening one presented in 1849. The residents of Reed street, in 1854, enjoyed as great an exemption from cholera as those of the most beautiful and attrative localities of our city. Why was this? the same order of people resided in Reed street in 1854 as resided there in 1849. The condition of the houses, as regards light and ventilation, was the same at both periods. But the causes of a foul and noxious atmosphere had been removed, and, therefore, the elements were not present, in 1854, upon which the materies morbi of cholera could act, and, by its action, multiply itself, and by that multiplication, increase the number of its victims.

What is true, I think, in regard to the manner in which the materies morbi, the disease-generating element of cholera, multiplies itself, is equally true in regard to the materies morbi of yellow fever. Let us see. In the summer of 1853 yellow fever made its appearance in the neighborhood of South street wharf. At that time the culvert emptied itself into South street dock. The wharves in proximity to South street were covered with mud and animal and vegetable matter to the depth of

several inches. The locality was very filthy, and the atmosphere was loaded with foul gases generated by the decomposition of filth in the dock and on the wharves. Every case of yellow fever which occurred in 1853 has been fairly traced to some connection with South street wharf. The fact is uncontrovertible, that the yellow fever poison was in the neighborhood of South street wharf, and there only. Why did not yellow fever make its appearance elsewhere, if filth in the presence of earthy matter, and certain thermal and hygrometric conditions, are all that are necessary to produce that disease? Why was it confined to South street wharf, when there were other localities along the river front, where culverts emptied their thousands of gallons of the foulest of matter daily, and wharves also in the vicinage were equally as filthy as those in the neighborhood of South street. The same thermal and hygrometric conditions were present in those localities as were present at South street. Yet with the same terrene, thermal and hygrometric conditions prevailing, yellow fever was absent. Baker street, vastly more filthy than South street wharf—Baker street, the great depot of bones and rags gathered from the streets and gutters—Baker street, with its denizens of all hues from "sooty black to cherry ripe"—creatures as low in the scale of morals and human degradation, as men and women can be, even they were as exempt, with all the excessive filth which was gathered about them, as were the residents of our rural districts, from yellow fever. Why was this? There certainly existed in and about Baker street, in the summer of 1853, the terrene thermal and hygrometric causes said by some to be the only elements out of which the yellow fever poison can be developed. And here, too, were, of all persons, those who may truly be regarded as among the most predisposed to any such disease. Yet not a single case of yellow fever was developed in 1853, from the malaria of Baker street. How can you account for the exemption of all the filthy localities of Philadelphia from the ravages of yellow fever in 1853—except South street wharf and its immediate neighborhood? There certainly must have been something peculiar to South street wharf—something which did not exist at the other filthy localities, and which must have played a most important and grave part, withal, in producing the yellow plague of 1853. And was not such the case? He would answer yes. The Mandarin, a foul vessel from a yellow fever port, on board of which a death from yellow fever had occurred before her arrival, was there—and from her hulk, her festering, pestiferous hulk, there flowed into the impure atmosphere of South street wharf the miasm, the materies morbi of yellow fever, and that miasm found in that impure atmosphere the elements out of which to multip y itself, and thus multiplied most fearfully, did it deal its blows of death and consternation among those of that ill-fated locality. It will not do for us to close our eyes upon all these facts, and composedly say, while we wrap ourselves up in some cherished hypothesis with which these facts are at a jar, that the Mandarin had nothing to do with the development of yellow fever at South street; that the fever, and her presence, were mere coincidents. He hoped such sad coincidents would not again occur. This doctrine of coincidents is dangerous; it is fraught with a host of the most serious consequences. Its tendency is to the abolishment of rigid quarantine measures, and points to the throwing wide open the flood-gates through which diseases may be introduced among us from abroad. If they pointed to Norfolk and Portsmouth, Virginia, and asked how he accounted for the origin of the terrible yellow fever epidemic there, he would answer—what the Mandarin was to South street wharf, the Franklin was to Norfolk and Portsmouth. The Franklin there, as the Mandarin at South street, presented the miasm to a festering atmosphere in which it found the elements it required, to so group and arrange as to produce its like—the yellow fever materies morbi. He felt free to declare, that if the Mandarin had been moored at some other of the filthy localities of our river front, instead of at South street wharf—yea, had it been possible to have placed her beside Baker street, then would the other localities—or Baker street and not South street wharf—have been the place in which the yellow fever plague in 1853 would have developed itself, and there, as it gathered force and power, struck down its victims.

DR. CONDIE said:—In 1832 two portions of the British army marched across a desert in equal lines, but some few miles distant from each other. So hot and dry was it, that the bodies of those who died did not decompose, but became mummified. There could not have been there any decomposition of animal and vegetable matter, yet one of these portions of the army had cholera, and was decimated by it; the other remained perfectly healthy. Can the gentleman (Dr. N.) answer how it was that one part of the army was attacked with cholera, while the other escaped?

DR. NEBINGER said: If Dr. C. means that he should answer him now, he would do so. The Doctor knows quite well that there are strata as well as currents of atmosphere: that tornadoes or whirlwinds pursue for many miles their course in certain narrow lines, frequently not more than a quarter to half a mile in width, their whole track strewed with uprooted trees, destroyed villages, and desolated homes; whilst outside of this narrow line there existed no evidence to show any atmospheric disturbance. Vessels heavily laden with human freight have quitted Liverpool when there was no cholera there, and have, when in mid ocean, had cholera

suddenly to make its appearance among the passengers. It is conceded that such vessels have encountered a current of atmosphere loaded with cholera poison, and from it the disease has sprung up. It is a fact, also, that in such cases the disease has subsided quite as suddenly, doing so, doubtlessly, because the vessel had quitted the current of poisonous air. Such, indeed, is the case almost universally with epidemics. They travel in certain lines. They are pestiferous tornadoes, marking their course with death and desolation. Such instances of the development of epidemic diseases are facts which strengthen the hypothesis for which he contended.

DR. JEWELL, in reply to Dr. Condie, remarked that it was true, vessels had frequently arrived at our wharves and at the Lazaretto, during quarantine, from tropical ports where yellow fever was prevailing, and had lost some of their crew by the fever on the voyage, and even after their arrival at the Lazaretto, and yet had not introduced the disease; and why? because the condition of the atmosphere was not as propitious for the development of yellow fever some years as in other years—there is wanting that degree of heat and moisture to produce the fermentation and decomposition necessary to constitute what is called a distempered atmosphere. The arrival of the Mandarin in 1853, July 13th, was during that peculiar terrene condition of the vicinity of South street wharf, from filth and impurities of all kinds, acted upon by humidity in a temperature at mid-day not less than 79° or 80°, and when there existed a more favoring state of things for the development of the fever. It cannot be denied that the captain and mate, who slept on board the Mandarin from the time she arrived until she was discharged, and even up to the time of their attack, both had yellow fever, of which the mate died. Two of the crew died on the voyage home, from fever. While she lay at Lombard street, after leaving South street, where the fever first showed itself, besides the captain and mate of the Mandarin, Captain Robinson, of the brig Effort, lying in an adjoining dock, his mate, steward and a seaman, all sleeping on board—together with the mate of the Mazeppa, lying alongside of the Mandarin, together with some nine others residing on the wharf between South and Lombard streets—all suffered from the yellow fever, between the 19th and 27th of July. These were the first cases. From this time cases continued to occur in the adjacent streets of the neighborhood. If, as is admitted by Dr. Condie, the poisoned atmosphere of a yellow fever ship may spread in a city to a limited extent, what are its bounds? If it is capable of spreading 100 yards from the source of the infection, why may it not, wafted by the wind or under peculiar states of the surrounding atmosphere, infect a more extended space? He meant by the state of the atmosphere, a condition to re-produce the poison or to increase it, whether by fermentation or other chemical re-action of the elements of the air upon the poisoned air from the vessel, he would not pretend to say. Again, Mr. President, if, as the gentleman has told us, no epidemic of yellow fever has ever existed in Philadelphia—with which opinion all of us cannot agree—then, Sir, the argument that the fever may be introduced and spread from a foreign poison is still more plausible. Hence, in answer to Dr. Darrach, that if yellow fever originates at home, why is a quarantine necessary?—It is to prevent foul vessels introducing the disease into our ports—not by contagion, but by means of an infected atmosphere.

At Memphis, in 1855, steamboats from New Orleans introduced the disease, and thus a large portion of the city was infected by contaminating the surrounding atmosphere.

DR. WALLACE asked if it had ever prevailed in Philadelphia not co-incidentally with the arrival of a vessel infected with it.

DR. JEWELL replied that it had, as no vessel had arrived in 1801 nor 1826, or in 1858 in Kensington, to which the disease could be traced. There was, in these instances, as far as could be observed, no foreign agency. In the latter year it occurred during the foul condition of the water in the Kensington basin, with which you are all familiar.

ACADEMY OF MEDICINE, NEW YORK.

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The great discussion on Quarantine, which had been laid over from the last meeting on account of the inclemency of the weather, that had prevented many members attending, took place last Wednesday, October 5th, an unusually large number of members and others interested in the subject being present.

After the preliminary business, DR. GRISCOM opened the discussion on the preamble and resolution[1] offered by him at a previous meeting, by reading a paper, substantially as follows:—

It is not with the expectation of adding anything to the stock of knowledge of the members of the

[1] Whereas, The National Quarantine and Sanitary Convention, held in this city in the month of April last, adopted a resolution in the words following, to wit—

"*Resolved*, That, in the absence of any evidence establishing the conclusion that yellow fever has ever been conveyed by one person to another, it is the opinion of this convention that personal quarantine of cases of yellow fever may be safely abolished; provided, that *fomites* of every kind be rigidly restricted."

—Therefore, *Resolved*, That the opinion expressed in said resolution is concurred in and adopted by this Academy.

Academy, or from any claim to a practical intimacy with the character, habitudes or treatment of the great scourge which is the subject of the resolution now under discussion, that I venture to occupy a few minutes of time upon it. Well may inexperience shrink from any participation in the management of this almost uncontrollable subject; and well may he be considered presumptuous, whose unproven hands shall be stretched forth to uphold an ark which it would seem to be within the province of the high priests of medical science alone to touch. But as the mover of the resolution, in opening the debate upon it, I desire opportunity merely to express a few thoughts which have occurred to me in relation thereto, and to present, in as brief a manner as possible for me, the reasons why I shall give an affirmative vote upon the proposition before us.

It is unnecessary, in this place, to spend a moment in depicting the *magnitude* of the enemy of the human race, whose traits we are now to discuss, and whose mode of advance and attack is the chief question before us. It will suffice for this, to remember,

1st, That it has destroyed in some places, during its brief epidemic visitations, 3 in 7 of the population; and

2d, That the mystery of its movements has been such, that a century of observation and study by the acutest medical minds of the civilized world, has not sufficed for the settlement upon an indisputable basis, of its laws and philosophy.

It is probably safe to say, that of the entire catalogue of "the ills which flesh is heir to," this stands above all, in the amount, the extent, and to use a gentle phrase, the *animation*, with which its various points have been discussed by philosophers, both medical and non-medical, from the time of its first appearance upon the globe, to the present hour. Whether we regard its true nosological position, its topography, its type, its symptomatology, its pathology, its diagnosis, its prognosis, or its etiology, these have excited interest in professional circles to such an extent, that the Bibliographic Record of our fellow countryman, Dr. La Roche, enumerates the titles of 991 books and essays, consulted by him in the preparation of his own voluminous and erudite work.

On the present occasion, under the terms of the resolution before us, one only of the numerous points of this knotty subject is presented for discussion, and to that I purpose to confine my remarks. The resolution declares in effect, that yellow fever *is not personally communicable*. In its own language, it says, "in the absence of any evidence establishing the conclusion that yellow fever has ever been conveyed by one person to another." Mark the precision of the language—"evidence, *establishing* the conclusion."

We have had presented to us some evidence to the contrary, but it was declared by Dr. Benjamin Rush, just 60 years ago, after the epidemic of 1799 in Philadelphia, that "out of upwards of 1000 persons who have carried the disease into the country from our cities, there are no more than *three* or *four* instances to be met with of its having been propagated by contagion."

If so important a question is to be determined upon testimony so rare and disproportionate as this, where the witnesses *pro* are only three or four to the *thousand con*, surely we may be allowed to hesitate before deciding it against the liberty of the subject of the disease. But Dr. Rush was at that time a non-contagionist, and we may therefore be cautious respecting his statements. Yet if we examine the testimony adduced in favor of the personal communicability of the disease as furnished by its advocates themselves, we shall find it lacking in much that is essential to a satisfactory solution of the question. Such an examination I do not propose now to make in extenso;—abler critics have passed their judgements upon the various items. But one point I may mention, as presented in the cases cited by that ablest of American contagionists, the late Dr. David Hosack, as worthy of special attention. I refer to the entire absence of any allusion to the possibility of *fomites* having had any part in the production of the disease in the cases quoted. In the instances referred to by Dr. Hosack in his paper on the "laws of contagion," in the transactions of the Literary and Philosophical Society, from Rev. Dr. Moore, and Dr Caspar Wistar, this deficiency is most marked, and the personal contagious property of the disorder in those instances is assumed without any inquiry into the probability or even possibility of the clothing and effects of the patients having been the vehicle of the poison to others; and this too, though the patients had fled from infected districts after a residence long enough therein to have imbibed it into their own bodies, and may reasonably be presumed to have carried their clothing and other effects with them in their flight.

On the other hand there are strong grounds for the opinion, in the Germantown cases reported by Dr. Wistar, that clothing from the infected city might have been, or was, the vehicle of the disease; for it is distinctly stated, that the mother, whose daughter had lived in the infected portion of Philadelphia, and died of the fever in a few days after getting home,—that this mother, Mrs. Johnson, had "assorted the clothes of her deceased child," and afterwards took the disease and died. Now, although the case of this mother is accepted as one of personal contagion, the existence of the child's clothing *as fomites* is wholly ignored, and the same oversight occurs with regard to others who died at the same house about the same time.

Without a full investigation upon this point,—without what our legal friends would denominate a "cross-examination," testimony of this kind is, to say the least, only negative, and from the extremely small number of cases relied on, and especially in view of the additional thousands, who since the days of Rush, have been sick with the disease in uninfected districts, without communicating it to others, we are driven to the conclusion, that positive testimony of the personal communicability of yellow fever is utterly insufficient, in fact almost *nil*.

Next to Dr. Hosack, perhaps the most ardent and unflinching of the advocates of the contagious property of yellow fever, is his pupil, our highly esteemed and most worthy associate, Dr. John W. Francis, our regard for whose personal and professional character demands for all he says the most attentive consideration. I would therefore occupy a few moments in paying my respects to one of the cases relied upon by him as evidence of the doctrine which he upholds. It is his own case which he has related to us upon this floor, and also before the late Quarantine and Sanitary Convention. That I may not mistake the case, I quote all that relates to it from the report of his speech before that convention, as revised by himself. It is as follows:

"Of the fever of 1795, I shall state my own individual case to illustrate its communicability. My father contracted the disease in October of that year, and died on the 23d of that month. The day before his death I was seized with the pestilence. I could never learn that the infected district had invaded that part of the city, in which was our residence, near the head of Pearl street, nor that I had, as a child, aged six years, wandered at all from the premises. Personal communication about the sick-bed of my father, had engendered the disease. I was informed that my prescriber was Dr. Wm. Pitt Smith, who died shortly after, of yellow fever. I narrowly escaped, and well remember my coffin set up in the corner of the room; for in those days of dismay the patient was scarcely dead before interment took place; you were hardly cold before the grave received you; such was the dispatch with all classes of citizens."

To this recital the same objection lies as against the others, that nothing is said about the *possibility* of the disease having been communicated from father to son through the clothing brought by the former from the infected district, where he doubtless imbibed the poison, which he carried home with him, and caused his own death.

So also the case, reported by Dr. Fenner (in the Transact. Amer. Med. Association, vol. vii., p. 539,) which is the only one quoted by Dr. Dickson in the second edition of his Elements of Medicine, p. 274 in support of his opinion as to its contagiousness, in the following language: "A Mr. McManus, who lived on the hills of Hinds Co., more than 40 miles from Vicksburg, went down to that city and remained there several days in 1841, when it was severely scourged by epidemic yellow fever. Returning home he was soon attacked by yellow fever, of which he died after throwing up black vomit. A few days after his death, his wife, who had nursed him assiduously during his illness, and had not been away from home, was attacked in a similar manner, and died with black vomit and yellowness. Their residence was in a high and healthy locality, where yellow fever was never known to prevail. Now, it is evident," says Dr. Fenner, "that Mr. McManus contracted his disease in Vicksburg; but if *any one can doubt*, that Mrs. McManus caught the same disease from him, I must *confess it is more than I can do*."

Now here again nothing is said about the infected clothing or merchandise, which the patient carried home with him, and yet upon such *negative* testimony, Drs. Fenner and Dickson yield all doubts.

In illustration of the absurd extent to which the sentiment of the contagiousness of yellow fever will sometimes be carried, and as an offset to the case just given, I will quote the case of my own father, who was attacked in Philadelpbia in the disastrous epidemic of 1793. He was pursuing his mathematical studies, when the disease appeared. I give his own account:

"I continued until no one but myself remained, and until my teacher himself was seized with fever and confined to his bed. He considered his recovery, I apprehend from the beginning, to be doubtful, and furnishing me with the outlines of his will, desired me to prepare it in due form. Having assisted him in signing it, and perceiving him to be very ill, I was induced, although very desirous to remain with him and assist his wife in nursing him, to yield to their advice and leave the city. . . . . I embarked in a sloop for Alloway's creek, and spent one night on the water, and on the following night I was seized with a heavy chill, followed by high feverish excitement, which was considered as an evident attack of the disease from which I had fled. My parents, eight miles distant, were informed in the morning of my situation, and no time was lost in sending for me.

"My situation excited much sympathy in the town of Salem, (N. Jersey,) where I was pretty well known, and the attendance of my physician, who resided in the town, was very assiduous, until the fears of the inhabitants for his and their own safety, laid him under a positive interdict, as I grew more and more ill, no longer to enter my room or to go inside of the house. The very atmosphere of our dwelling was considered to be infected, so that travelers along the road, apprised of my illness, would climb over the fences and make a wide

circuit, to avoid the danger. But my physician, Dr. Rowan, though prohibited from seeing me, or coming to the house, continued his daily journeys to a tree, above a quarter of a mile distant, where he met my father, learned the symptoms, and prescribed what he thought best. The disease terminated in a dysentery, which being suitably treated, I began rapidly to recover, and was soon restored to health."

No other person in the house took the disease. Seeing then such omissions and admissions as these, may we not concur in the idea expressed in the resolution, as to the absence of evidence "*establishing* the conclusion that yellow fever has ever been communicated by one person to another." How *can* any one maintain the idea of contagiousness upon such evidence as this?

In the next place, I would present a single argument against the *possibility* of yellow fever being a contagious disease. This argument I have seen alluded to only in two works—that of Prof. Smith on Epidemics, and that of Dr. La Roche on Yellow Fever—and yet, in neither, in my judgment, is the argument pushed so completely to its conclusion as it appears capable of being, and it seems, to my mind, that had it been presented in its full effect, the mooted question might have long ago been settled.

To understand fully the nature of the argument, we must first know the real meaning and application of the word "contagion," as now generally received.

Whatever may have been the views of the earlier medical philosophers, as to the precise meaning to be given to the word, or however it may have been confounded with infection, the modern application of it is clear and precise. Nowhere, probably, shall we find a more distinct and comprehensive definition of it than in the first four lines of the body of the work entiltled, "Elements of the Etiology and Philosophy of Epidemics," by Joseph Mather Smith, M. D. This definition is in the following words:

"*Contagion is a poison generated by morbid animal secretion, possessing the power of inducing a like morbid action in healthy bodies, whereby it is reproduced and indefinitely multiplied.*"

Accepting this definition, we perceive, 1st, That it is a *secretion*, (not a mere excretion,) the product of a *living animal action;* 2nd, That it is a morbid product, i. e., the result of a diseased action; and 3rd, It possesses the power of inducing a like morbid action in healthy bodies, and of multiplying and reproducing itself.

Applying this definition to certain well known diseases—small pox for instance, or measles, or scarlatina—and we have a full and distinct comprehension of its different points. The virus of variola, whencever it came originally, responds completely to them all. It is a poison reproduced in and secreted by a *living*, not dead, or decomposed tissue; it is an animal and not a vegetable production; infused into a healthy body, it reproduces *small-pox et præteria nihil.*

By the same author, contagion is contra-distinguished from infection by applying the latter to the *product of the decomposition of animal and vegétable substances*, taking the same view of the distinction as did Dr. Richard Bayley in 1796, and although other authors differ as to the most appropriate application of the word infection, some, as Wilson Philip, and Dr. Bateman, the supposed author of the article, "Contagion," in Rees's Cyclopedia, appropriating it to "the *act of communication* of the disease;" yet the definition of the word contagion, as given by Prof. Smith, is the almost universally accepted one, and, with few exceptions, the same may be said of infection, and is evidently the most free from objection.

But, to be the product of *decomposition*, the *vitality* of the decomposing substances must first be destroyed. No time need be spent in the elucidation of these points, since they have been ably discussed by some of the authors alluded to, and I content myself with the remark, that these definitions and distinctions appear to me to constitute the really correct interpretation of these pregnant words.

Let us then apply them to the case before us. The first question which arises, is this: Admitting the broad distinction thus drawn between these two sources of disease, contagion and infection,—the one the product of living animal action, the other the result of the decomposition of dead animal or vegetable matter, or of both combined,—can these two classes of poisons, so distinct in their chemical and physiological characteristics, produce the same results, when absorbed into the healthy body? Are they convertible influences?

For example—can the *marsh miasm*, which is the source of intermittent fever, by passing through the living body, cause the elimination of a poison capable of reproducing itself, or the miasm which is the cause of the more pernicious remittent, from Panama and other parts of the Isthmus, which for several years past has crowded our hospitals, did it ever, or can it, by being distilled through the animal alembic, generate a poison capable of producing the same disease in another. Every one knows to the contrary.

It may be stated as a medico-etiological axiom, that *two different and distinct causes cannot produce the same effect.*

Now, what is the original source of yellow fever? I say the *original* source—is it a secreted animal poison, generated by morbid animal action? On the contrary, it is universally accredited, even by the advocates of its contagious power, to a poison derived from vegetable putrefaction. By those who believe it to be exclusively an imported disease, as

by those who consider it wholly of domestic origin, as well as by those who regard it as both exotic and indigenous, its original ultimate source is thought to be no other than vegetable decomposition, terrene exhalations, or both combined, aided and controlled by certain meteorological circumstances. Not one writer, within the range of my observation, has ever attributed it to an animal poison secreted by the living body, and probably not one could be found, even in the ranks of the most decided contagionists, to hazard the expression of an opinion to that effect. If, then, it has no other origin than vegetable putrefaction or terrene exhalations, it cannot, according to the axiom before announced, have any other source; and, furthermore, admitting, for the sake of the argument, that a body laboring under yellow fever produces any secretion, or aura, or poison in any shape, (though this has never been proved,) that poison has a living animal, and not a dead vegetable origin, and it cannot produce the same disease as the original poison produced; and it must, therefore, if anything, produce something *different from yellow fever*, which is a specific disease, the result of one specific cause—*Quod erat demonstrandum.*

Such, Mr. President, is, I think, the true philosophical basis upon which this question should be discussed. If the principle which I have stated, viz., that two different and distinct causes cannot produce the same disease, is not merely an apothegm, but has the force of an axiom, as I claim for it, then there is an end to this long waged dispute respecting the contagious property or capability of yellow fever.

But is it so? I have been as yet unable in my brief reflections to find an exception to the rule, and moreover I am inclined to the belief that this law is in accordance with the views of the teachers of medicine generally, though it may not have been heretofore so epigrammatically stated.

But I find this proposition met at the outset by another which has had some currency among medical writers and teachers, and which is distinctly enunciated in the following passage in the speech of Dr. Francis, already alluded to:

"The complexity of the assimilative powers," says Dr. F., "tells us of man's subjection to noxious agencies, and that while the laws of life enable him to hold in abeyance to a certain degree, specific causes, inexplicable evidence compels us to recognize the generating development of contagion and infection in his own individuality. We are forced, therefore, to admit the contagious principle more or less intense *in fevers of a continuous nature*, and yellow fever is one of those disorders, and hence communicable from one person to another."

And in another place he remarks: "It falls not within the category of small pox; and ship and typhoid fevers may prove more fatal to medical attendants; but if we discard the quality of contagion, we rob continued fevers of one of their characteristics."

If this be true,—if continued fevers of whatever specific character, are *ipso facto*, generators of a contagious principle, then indeed must the other law which I have stated above, be null and void; in fact no law at all. What may be the particular ground upon which this law, as given by Dr. Francis, is assumed to stand, I confess my inability to perceive,—and I cannot doubt that on a hearing of the argument upon it, the profession would, as the Court for the Correction of Errors, pronounce it unconstitutional, or at least, after the Scotch fashion, *unproven.*

In the absence of sufficient evidence to the contrary, having by this process of reasoning satisfied myself, at least, of the non-contagiousness of yellow fever, and being convinced also that it has its origin here solely by importation, there remains the question, "*how is it propagated?*"

This question, though pertinent to the general subject, has not a sufficient bearing upon the resolution before us, to justify the expenditure of much time upon it. I can only say, therefore, that I regard the introduction of a ship load of the atmosphere of an infected locality, as of Havana or New Orleans, when the disease is rife there—as the sowing of a seed,—our own atmosphere may be prepared for its germination and growth, or it may not. If not so prepared the seed has fallen upon barren soil,—but if it is so prepared, than it has fallen upon good ground.

But I am further of the conviction that like good farmers, we may not only prepare and enrich the atmospheric soil for its reception and growth by manuring it with domestic guano in the shape of street filth, foul yards, bad drainage, but particularly by domiciliary crowding, ill-ventilation, and the absence of proper sanitary supervision,—but that we have it in our power also like good sanitarians, to diminish its fertility to a great degree, and deprive the germ of its pabulum.

Prof. Smith, while regarding the disease as sometimes a home production, produced by an exhalation "from masses of public filth, and soils containing putrescent matter," at the same time admits, that it requires for its generation "a high range of temperature and *certain epidemic influences of the general atmosphere.*"

This "epidemic influence of the general atmosphere" is the "good ground" to which I have alluded, in which the imported germ must be sown to be propagated, but in what that influence consists is the great question to which the minds of sanitarians have of late years been devoted to the study of. Whether the whole secret has been revealed in

the highly interesting results obtained by Dr. E. H. Barton and his colaborers, of the late New Orleans sanitary commission, may be yet a mooted point; but that we had in 1856, a most striking evidence of the probable correctness of their theory, and also that a very narrow line does sometimes divide the borders of the barren and the fertile yellow fever atmosphere, as on that occasion, when the Hygrometers of Fort Hamilton and New York showed such a wide difference in the amount of atmospheric moisture, is a fact that cannot be overlooked, nor easily contravened.

But, in answer to the theory of assimilative fermentation, as of a spark thrown by importation among combustible materials in a city, and reproducing itself, Dr. Smith maintains that "such a process cannot account for the *slow* and *progressive* manner in which cities usually become pestilential;" that "the poison emitted from infected vessels and their cargoes, together with the noxious exhalations from the soil, is swept away by every passing breeze, and consequently it cannot gradually extend by a process of assimilation." This appears to me eminently unsatisfactory and inconclusive. Nor is it any more satisfactory, as further stated by the same author, that "if an assimilative fermentation can really take place, it is obvious that the air of a *whole* city would very soon become pestilential; for the miasm, being scattered by the wind through the streets generally, would quickly reproduce itself alike in every quarter." It is incorrect, because the process is not necessarily a *quick* one; it is probably and doubtless subject to some law of growth and increase not yet understood, depending upon temperature, moisture, electricity, ozone, or whatever yet undiscovered principle may exist in the atmosphere, which vary from day to day and from hour to hour—at one moment rife and prolific, at another curtailed and altered. No more need it be *quick* to spread, than that a child should be quickly raised to manhood—an acorn to an oak, or a larva to a fly; and that *whole* cities are generally not subject to the pestilential power, (while it is admitted that the poison does spread gradatim,) may be accounted for by the fact that some are larger than others, and the poison can continue to spread only so long as the necessary circumstances exist in the atmosphere—as long as the heat and moisture are sufficient. Fortunately for us, these are not perennial, and a single touch of frost instantly destroys the morbific principle, whatever it is.

Now, in all this, there is nothing incompatible with the idea of an assimilative operation—or rather, I would say, with the growth of a principle—call it fungus, algæ, animalculi, miasm, or what you will, as long as the favoring circumstances exist.

Says Dr. Smith, "The poison emitted from infected vessels and their cargoes cannot gradually extend by a process of assimilation, because it is swept away by every passing breeze." Carrying out this idea to its fair and legitimate extent, we might assert that no poison of any kind can lurk in the air. Let the microscopic bladders of the genus *nostoc*, with which our gifted Torrey recently calmed the excited populace, teach us a lesson of wisdom—that if the laws which govern the coming and going of water growths, with intervals of years greater than the average length of human life, are so entirely beyond our comprehension—so may the still more subtle and intractable influences of the invisible atmosphere give growth to powers which are none the less real because beyond the detection of any instrument or test yet known to us.

That there is an entity, a corporeal substance, or at least an active principle, poisonous in character, and yet physically undistinguishable by any human means, is admitted; its effects are too apparent to be denied. That it possesses sufficient materiality to be absorbed by, or attached to certain porous substances, though this property is denied by a few, is acknowledged by the author of the "Elements" himself. Why, then, should it be denied a power of growth, of increase, of progression, of extension? If the grosser "air plant" suspended by a thread or wire, or the delicate fungus attacked to the naked rock, will grow upon atmospheric food alone, we may easily believe that an invisible, intangible material may also there find a pabulum.

The resolution before us concludes with these words:—"*Provided that* FOMITES *of every kind be rigidly restricted.*" This covers all the ground left unoccupied by the body of the resolution itself. But the question here arises—*what are fomites?*—a question equally interesting in a literary as in a medical point of view. What say the dictionaries?

Dr. Parr, in his compendious work, gives the following definition:

"*Fomes*, fuel, when spoken of in diseases, is the remote cause of the disease; most commonly the material efficient cause. In the plural, *fomites*, it is generally applied to the infection contained in woolen or cotton, and rendered more deleterious by confinement."

The word here used, it will be observed, is *infection*, and fomites is the infection *contained in* the goods.

Dunglison's definition is:

"*Fomites*, from *fomes* 'fuel,' any thing which retains heat. A term applied to substances which are supposed to retain contagious effluvia, as woolen goods, feathers, cotton, etc."

This language taken in its strict sense, is certainly obnoxious to criticism; substances *supposed* to contain contagious effluvia being made fomites.

Hooper says "*fomites*, a term mostly applied to substances embued with contagion," thus restrict-

ing the poison to *contagion*, but requiring the vehicle to be actually embued with it; while Reese in his Lexicon in copying from both, copies the errors of both, and says, "*fomes, fomites*, terms applied to articles supposed to be embued with contagion."

The language used by Dr. Pereira Gardner, in his Medical Dictionary, is still more objectionable. His definition is, "*fomites*, plural of fomes. *Fomes*, fuel. In medical language a fomes means a porous substance, capable of absorbing and retaining contagious effluvia. Wool and woolen cloth are among the most active fomites;" so that the coat I now wear, is a fomes, because *capable* of absorbing and retaining a contagious effluvia.

It is remarkable that there should be such a difference in the language of medical lexicographers, a difference so great, that a student of medicine who desires to be classically accurate, might be not a little puzzled; for while Parr applies the term to the infection *contained in* the woolen and cotton stuff, Dunglison and Hooper apply it to the stuff which *contains*, or is embued with it, and Gardner to a substance *capable* of absorbing and retaining it.

To obtain a correct and exact appreciation of these words, we must go back to the original, and we shall there find that the idea which underlies the whole is that of a *cherisher* or nurser, and in its literal meaning, it is, in the singular a *spark*, or kindler, and in the plural, that which aids and assists, as kindling-wood, touch-wood, or tinder. Thus Festus, a learned author of the 3d or 4th century, on the "signification of words" says the sparks are called *fomenta* which are struck from red hot iron;" and Andrews, in his Latin and English Lexicon, has the following passage:

"FOMITES *sunt assulæ ex arboribus, dum cæduntur excussæ*—fomites are the splinters or chips shaken from trees while being cut down.

Fomes then, in its medical sense, is the poisonous spark, which wants but the opportunity to exercise its power, and is cherished and protected by being retained within the folds or pores of some appropriate substance, and the two together constitute the *fomites*.

But it is claimed by Dr. Francis that the human body is not only the generator of the contagion, but that it may be itself also the vehicle of the poison to others. The following passage from his speech, from which I have already quoted, interests not only by the eloquence of its expression, but also by the boldness of its sentiment. Addressing the convention he said:—

"You say in your contemplated resolve, there is no evidence of the personal communicability of yellow fever, independent of fomites, by which I infer you admit its contagious or infectious nature, when such agencies are present. How are you to liberate the afflicted patient from the action of fomites, or the by-stander who occupies his room and surrounds his bed? Everything, it is suggested, is to be removed; if there be a deficiency of electricity, I suppose it is to be restored; the patient is to be divested of all apparel; and thus, with proper ablutions, you say he becomes innoxious. The clothing which he impregnated with the emanations of his pestilence, is to be carried out of sight. But of the patient himself, the immediate source of all the poison, the generator of the fomites, what of him? Shall every tangible portion of his body, where the evil may lurk, be subjected to certain nameless operations? Is he to be shorn? Is he to be denuded? If a bank-bill that has had some currency among many hands, can circulate the fever by its transmission from one person to another (as has been asserted by the very authority which denies contagion,) what is to be the lot of the patient himself, the fountain of all the evil? In other words, gentlemen, you tell us, remove every source of contagion, and then the patient is unable to spread yellow fever. Pardon me, if I think I am warranted in saying, this doctrine is neither deferential nor inferential, nor consequential. It is a negation. Well, after all, thus safely ensconced in the ark of safety, little may be dreaded of the troubled elements. A particle of these wonderful fomites, even of an impregnated bank-note, may spread desolation and death; the corrupt or diseased patient, the source of all the danger, is, however, an impotent and harmless mass. If this is the theory espoused here, I am strongly reminded of a nursery couplet that I early learned!

> South Hampton's wise sons found the river so large,
> Though t'would carry a ship, t'would not carry a barge.

To constitute *fomites*, as I understand it, there must be an absorption of an external poisonous principle by a porous substance, and the hold upon which must yet be so slight that it may be immediately thrown off when exposed to the air, so that it may be inhaled or absorbed by a human being, in whom it may produce its specific morbid effects.

Now, is it credible, as is intimated in the passage above quoted, that the living human body can become such a fomes?—that it can absorb into its texture, like a blanket, the miasm of yellow fever, and emit it again when approached by a healthy subject? The question conveys its own answer.

To employ the illustration of the couplet, a ship retaining within its hold, an infected atmosphere, may be regarded as fomites, while the living bodies of the passengers cannot be so considered.

The language of the *proviso* is to be taken in its strict literal sense; if the infection lurks under the nails, pare them down; if in the hair, shear it; but a simple ablution, in my judgment, will suffice to

disinfect the entire body, rendering it harmless, even though itself laboring under the morbid effects of the poison.

Such, Mr President and gentlemen, are some of my reasons for the vote I shall give in the affirmative on the resolution which was passed so nearly unanimously by the late Convention.

After Dr. Griscom had finished the reading of his paper,

Dr. Reese obtained the floor, and stated that he had prepared a paper on the subject. He then proceeded to read it in his usual emphatic manner; its chief points were as follows:

He first considered the introduction of this resolution as an anomaly; it was wholly unprecedented, in his opinion, that a National Convention, should in person, by its President, solicit the endorsement of a local body for any opinions which they, on their own responsibility, saw fit to promulgate. He granted in the present case, that there was a sad need of endorsement by *somebody*. The late action of the Sanitary and Quarantine Convention had, he claimed, been renounced and repudiated by *every* medical journal in the land which has the authority or qualification to speak upon the subject. But he must confess his amazement at the temerity of an attempt to bring a resolution before this body, the passage of which would only stultify the Academy itself; a resolution which he considered intrinsically absurd, and at variance with science and common sense.

He regarded the declaration in the resolution, that yellow fever was *not* contagious from person to person, but contagious *fomites* or things, as a contradiction and absurdity. This was the gist of the resolution divested of its ambiguity and circumlocution. How could the clothes, bedding, etc., of a patient communicate the disease to a well person, when the sick body itself, the source of the malady, did not? This is not merely transcendental mysticism, but sublimated *absurdity*, and he would submit with great deference—"*arrant nonsense*"

Secondly, Fomites could not be predicated to exist, and never were alleged to exist, except in the presence of contagion. At the time of Parr and the earlier writers, no distinction had been made between contagion and infection, the terms then being used synonymously. Fomites were those agents which had received contagion from the body of the sick person, and these, being propagated among the healthy, produced disease. In the case of small pox, the danger from fomites is eminently great; yellow fever, however, not being contagious—according to the opinion of the Convention—the very idea of fomites was absurd. It asserts the existence of fomites, while denying the *contagiousness* of yellow fever—the essential element of fomites.

After a humorous allusion to the "Sepoys," at Staten Island, who had practically carried out the doctrine of the Convention, before it was promulgated by that body, by burning up all *fomites* at the island, the Doctor continued, that it was worthy of consideration, that the resolution, brought forward to be endorsed by this Academy, is at direct variance with a resolution passed by the same convention held the year before in Philadelphia, in 1858. This same convention, made up, to a great extent, of the same men, resolved, then, that yellow fever was communicated from person to person, and *not* by fomites. Is it New York *versus* Philadelphia? He would let the two conventions fight it out, Kilkenny fashion, until they have devoured each other.

He further continued, that although his individual opinions were well known to the Academy, he would take occasion to repeat, that he denied that yellow fever was contagious, either by persons or fomites, when removed to a healthy atmosphere, and he regarded the restrictions of quarantine, except for cleansing and fumigating purposes, as a relic of barbarism, worthy only of the dark ages, disgraceful to science and civilization.

Dr. R. alluded to instances of which he knew, when the medical officers connected with the quarantine had proved themselves incapable of making a diagnosis, whether patients had yellow fever, remittent or intermittent fever.

When a patient, whose disease had been first diagnosticated intermittent or remittent fever, was sent to the city, and died of yellow fever, they call it "bilious remittent fever of a *high grade*," to conceal the ignorance of diagnosis, and send the vessel on which he arrives, back from her wharf down the bay, and yet, with all this backing and filling, floating hospital and all, there has been no spread of yellow fever in the city. Meanwhile this carricature of a quarantine had been kept up, at an expense of hundreds of thousands of dollars, and a still greater loss to commerce, without influencing the sanitary condition of the city. The sanitary officers might as well be *absent* with Dr. Jerome, or at the bottom of the sea.

He would protest against the passage of this resolution, which had damaged our reputation more at home and abroad than any other action of any medical body. It had been passed by a convention, not representing the majority of the profession, but composed of the "odds and ends" of Boards of Health, the majority of whom had never seen a case of yellow fever.

Dr. Francis next took the floor. He remarked that he had waited a moment or two after the re-

marks of Dr. Reese, expecting that his venerable friend, Dr. Smith, would take this opportunity to address the Academy. He had listened with great consideration to the scientific paper of Dr. Griscom, which was marked throughout by that mental discipline which characterized the gentleman who had been so long devoted to, and was so intimately acquainted with the sanitary affairs of the city, and the paper to which the Academy had listened was both honorable to his head and to his heart.

But he had been too long acquainted with medical history and medical men, not to know that even the most assiduous and acute philosophers occasionally fall into error, and he could not for a moment believe, but that Dr. Griscom was laboring under a strong delusion. Upon this everlasting topic of yellow fever, upon which so much had been said and written, that its contemplation even now almost gave him the black vomit; he would pray most earnestly that this distinguished Academy would not hastily commit itself.

He regarded the resolution passed by the late Sanitary Convention, and now before this Academy, as a counterfeit issue, and God forbid that its members should aid in passing it. That the proposition was fraught with many absurdities, is unquestionably true, as that very astute man, Dr. Reese, had observed to-night. Let the whole matter be postponed, and *postponed*, and *indefinitely* postponed, until it shall have died away.

The theory was set forth elaborately in Dr. Griscom's paper, that fomites propagated the disease; and yet that the contagion is spread from the *patient* to these *things* called *fomites*, is denied Can there be anything more absurd! He was convinced that there is not a man in this Academy who can believe this doctrine of the Sanitary Convention. A great difficulty arises in combatting this theory, because no distinction can be made between fomites and the body itself; you cannot separate fomites from the body. Here is always a loop-hole to escape. When a case, in which contagion is incontrovertibly clear, is brought up—why, it is said, it was not the body which spread the contagion, but the fomites.

When the subject was discussed here on a former occasion, Dr. Smith came forward and quoted the authority of Louis (?) who reported to the Academy at Paris, that yellow fever was not contagious. But he did not consider him a trustworthy authority. When he was present in the Academy at Paris in 1816, and the report was made that yellow fever was *not* contagious, President Charles, at the same meeting of the Academy, gravely related that gout had been communicated to a man by taking from a clothes line and wearing, the stockings of a patient affected with that disease. How can you trust such authority as this!

He did not wish, however, to enlarge upon the subject; this subject could never be settled by discussions of this kind; it would be far better to postpone the matter. There was another circumstance connected with the subject, to which Dr. Reese had already adverted. You have done more, recently, by this discussion on yellow fever, to injure the medical profession, than can be imagined; and if you adopt such an extravagancy as this, you discard and throw into disrepute all the great names of the profession.

Dr. Rush had been cited in this discussion; but he is *no* authority on this subject. He may tell you, that the more you bleed the greater will be the probability of saving a man from hydrophobia, but the man, who believes in contagion in one year, and is a non-contagionist the next, cannot be confided in. This is like Armstrong. First it was *all* bleeding in puerperal fever, and then it was all cathartics. Reverently as he was disposed to feel toward Dr. Rush, he could not but believe, that the fact first promulgated by him cannot be altered. "A fact is an eternal thing," said Cicero, and as such it must remain unaltered.

If you adopt this resolution, you will commit this Academy, and perform what I call a *felo-de-se*. You are only following in the train of men who have never seen a case of yellow fever. Popular feeling is against you. When you state that there are fomites, you actually admit that yellow fever is contagious.

Dr. Jos. M. Smith followed next, and gave a clear and succinct definition of fomites, which he considered the real issue of the discussion. He could not see any difficulty in defining the word. He regarded the fact as settled, that there *is* such a thing as fomites. It had been stated by Dr. Griscom, that fomites meant material which may absorb poison, (or contagion, if you please,) and by that material may be conveyed to others, and reproduce disease by emanations. Fomites, then, in the general term, comprises any material that can convey disease of whatever character. The older physicians understood, by contagion, fomites, and infection, any agents capable of producing disease. If a miasm sprung up, it was called contagion—infection, the term, as then used, meant anything and everything pestilential in its character. The union of any substance, such as woolen, cotton, etc., with a morbific agent, produces fomites. The learned Doctor here cited numerous instances of diseases, which produced various kinds of fomites, such as rubeola, variola, scarlatina, &c.

There are, however, other poisons not developed in a vitalized body, but generated *without* the body, by noxious emanations, not of a specific character, such as the exhalations from the lungs and skin,

which, when condensed and accumulating on board of a ship, give rise to typhus fever, and though almost all the passengers may escape the infection, yet persons not acclimated to these fomites will be seized with the disease. Thus, also, a ship arriving from the West Indies, where yellow fever has been endemic, may carry the fomites of yellow fever, whatever their nature may be, with them, even though none on board may be affected with the disease.

Dr. Smith then discussed the question of the assimilative power of a yellow fever atmosphere, referred to in Dr. Griscom's paper at length, contending that in cases where it was not dispelled it might be slowly assimilated and propagated by the soil.

Dr. Mott, in a few remarks, expressed his belief that yellow fever, with which he has been perfectly well acquainted since 1804, was occasionally a contagious disease. He wished to know why physicians in this city and elsewhere had more fear of visiting a patient with yellow fever, than one with intermittent. He would oppose this resolution, so far as his vote was concerned, because he looked upon it as of a very injurious tendency.

Dr. Reese again took the floor, in order to explain his position. He was perfectly well aware that the term "fomites" was formerly applied by writers to agents, which we do not now regard as producing contagious diseases. They applied this term to those diseases which they supposed were contagious But in his opinion it was perfectly plain, that *without* contagion there can be *no* fomites. A ship coming from a pestilential, infected locality, carried the infection, not in fomites, but in its atmosphere; and this poisonous atmosphere it is, which produces the disease. He denied that there has been a *single established fact, which demonstrates that yellow fever has ever been communicated by fomites.*

Dr. Alfred C. Post regarded the arguments of Dr. Reese as in perfect harmony with the theory of fomites, and as contradicting himself. That gentleman had said, that yellow fever was propagated by the atmosphere of vessels coming from infected ports. These vessels then, according to the gentleman himself, can only be regarded as gigantic fomites. All those who advocate the theory of fomites, understand by that term any material which is capable of absorbing poison, and conveying that poison from place to place, and then giving it out so as to produce the disease in others. This does not convey any idea of contagion. Suppose it had some *chemical* cause; suppose it to be something of whose intimate nature we know nothing: in this view of the case, the ship is regarded as a fomes, although on a much larger scale than a trunk or a bale of cotton. The gentleman has *admitted* that the disease may be conveyed by a ship; now it does not matter whether the fomites be a thousand cubic feet or one cubic foot in size, so long as it is capable of communicating the disease. The members of the Sanitary Convention, he believed, understood the term in that sense. Fomites have no regard, then, to size.

This idea that fomites and contagion are inseparable, is a man of straw, which these men have set up to pierce with their darts; they have accused us of entertaining absurd doctrines, which we have no idea of entertaining.

The discussion was then postponed to the next meeting, and the Academy adjourned.

On the Employment of Perchloride of Iron in the Treatment of Purulent Wounds, (*Gazette Hebdom.* August 26.)—The perchloride of iron has the property, when in a very neutral and very concentrated solution, of not only coagulating albuminous liquids of any kind, but of arresting putrefaction also, and even acting as a disinfectant. White of eggs, albumen of the blood, and other albuminous liquids have been kept for several months without any trace of decomposition, after having been coagulated by this substance. To avoid the presence of the free acid, which is found in the solution usually used in hospitals, M. Terreil proposes the following formula for a solution, which has all the advantages, and none of the disadvantages, of the usual prepation:

Crystalized perchloride of iron,	1 part.
Distilled water, - - -	4 parts.

This solution may be employed as a hæmostatic, or may be diluted, and thus effects the coagulation and disinfection of the purulent liquids, which flow from wounds of a healthy character; and M. Terreil thinks that its employment, continued for a legnth of time, may not only relieve, but even cure such lesions.

Aconite in Traumatic Neuralgia.—Mr. Ussher reports the successful external application of Tincture of Aconite in a case of neuralgia resulting from a wound of the thumb. The patient had suffered intense pain along the course of the musculo spiral nerve, without relief from other remedies. The Tincture of Aconite Root was painted along the course of the nerve several times, and permanent relief followed.

EDITORIAL DEPARTMENT.

Periscope.

On Buccal Cancer among Smokers.—M. Bouisson, of Montpelier, has given to the world another blast against tobacco, in deducing the increased prevalence of labial cancer from its excessive and long continued use. He relates 68 cases affected with cancroid and cancerous tumors in persons addicted to smoking, and thence traces a complete pathological history of the disease. We will give a brief resumé of the arguments he employs to prove his etiological doctrine, remarking, however, that they apply rather to a cancroid disease than to cancer properly so called.

There is no doubt, says M. Bouisson, that cancer of the lip and other parts of the mouth has become more frequent in proportion to the increased number of smokers, and to the increased amount of smoking among those who had already contracted the habit; the disease has progressed with the habit. This lesion was quite rare in the last century. If we examine the works of the surgeons of that time, and even those of the beginning of the present century, we find that cancer of the lip is not noted as having any marked predominance over cancer of any other part of the body, and none of them seem to have suspected that tobacco could play any part in determining the disease. All the works devoted to this disease, whether by French writers or those of other nations, are silent on this point of etiology. Hence we are justified in inferring, that if such men as Boyer, Delpech, Bell, Richerand, Heister, Jourdain, etc., did not notice the frequency of labial cancer in the proportion in which we find it at present, it was because the occasional cause of the disease was less common than now, and the number of smokers being fewer, the number of diseases which this custom engendered was also less considerable. But now the increase in the number of smokers, *à outrance*—men who are never without a cigar or a pipe in their mouths—for whom even sleep does not remove the offending weed; this increase, he remarks, has also multiplied, in a remarkable degree, the number of cancerous diseases of the mouth requiring surgical operations. Never before was there so much talk, in hospitals and in private practice, of cauterisation, of excision of the lip, and of cheiloplasty. M. Bouisson is of opinion, besides, that something more than the mere habit of smoking is required to produce labial cancers, and accords a good deal of influence to a general diathesis; but he believes that he has good reason to maintain that this morbid disposition would remain latent but for the local exciting cause produced by the irritative action of the tobacco incessantly concentrated on one point. Besides the argument deduced from the increased frequency of labial epithelioma, the author brings the following reasons to his aid, viz: It attacks the lower lip most frequently, on which the pipe stem or cigar habitually rests; it is rare among women and children. M. Bouisson speaks of a woman who was affected, but she was an inveterate smoker, and the older the smoker the more common the disease, most of the cases being in men over forty years of age. Among the poorer classes, who smoke short pipes and bad tobacco, the disease is developed sooner than among the rich, who smoke delicate cigars and long pipes, and who neutralize the local effects of the combustion of the tobacco by hygienic means. The local action of the heat on the lips has probably some effect in the production of the disease, but tobacco alone would suffice M. Bouisson gives the case of a physician of Barcelona from whom he removed some epithelial vegetations in the nostrils, which he had no hesitation in ascribing to the practice so common among the Spaniards, of eliminating the smoke of their cigarettes through the nose, and it is thus, probably, that in many cases epithelial cancers of the tongue, cheeks, arch of the palate, gums, and tonsils are produced.

As M. Bouisson's work seems thus to be founded on serious and important considerations, smokers should take warning, and indulge but moderately in their luxury.

Physiological Chemistry.—Researches on the nature of the sugar formed by the hepatic glycogenic matter, by MM. Berthelot and De Luca:

We know, from the experiments of M. C. Bernard, that the hepatic glycogenic matter is capable of being transformed into a particular kind of glucose; but the nature and specific character of this glucose have not yet been determined with accuracy.

The experiments of MM. Berthelot and De Luca demonstrate the identity of the glucose formed by the hepatic glycogenic matter with ordinary glucose; that is to say, the glucose of grapes and diabetes.—*Acad. des Sciences, August*, 1, 1859.

PHILADELPHIA, SATURDAY, OCTOBER 8, 1859.

YELLOW FEVER AND QUARANTINE.

The subject of yellow fever as connected with quarantine, it will be observed, occupies a very large portion of this week's issue of the REPORTER. It is a singular fact, that although our country has been more exempt than usual from yellow fever the past season, this disease during that time has largely occupied the public and professional mind. Two or three reasons for this may be mentioned.

For some cause, without, however, any logical foundation, a notion has prevailed ever since the terrible visitation of yellow fever at Norfolk and Portsmouth in 1855, that the disease would next appear, epidemically, in the great cities of Philadelphia and New York. And this vague notion was probably a strong element in fomenting the quarantine difficulties on Staten Island last year, which resulted in establishing this year the system of floating hospitals for yellow fever patients. The same causes also gave the subject of yellow fever and quarantine, as connected with this disease in particular, an unusual prominence in the discussions that took place last April, before the Quarantine and Sanitary Convention, which met in the City of New York.

But, whatever may have been the cause, it is a fact, that two of the most important discussions on yellow fever and quarantine that have ever taken place in this, or perhaps any other country, have recently occurred before prominent medical associations in Philadelphia and New York. We have the pleasure of spreading full and accurate reports of both these discussions before our readers this week. We have delayed the publication of the discussion before the Philadelphia Co. Medical Society in order that we might present it in the same issue with that before the Academy of Medicine in New York. Unfortunately (for us) both discussions were very long, though, as will be seen, not the less interesting. To get the whole into our issue of this week has necessitated an addition of many pages to our regular issue, and the postponement of a large amount and variety of interesting material, which is awaiting an insertion. We doubt, however, whether any of our readers will regret the course we have pursued, when they find themselves possessed of so important an addition to the literature of yellow fever and quarantine. It is not often that the views of such men as Jewell, Condie, Bell, and Darrach, of Francis, J. M. Smith, Mott, and Griscom, on the same subject, are simultaneously sent out to the world.

In conclusion, we think that, without subjecting ourselves to the charge of vanity, we can ity, claim that, in the publication of these debates,—one of them having taken place in New York on *Wednesday* evening, and issued in this city on the next *Friday* afternoon,—we have inaugurated an era in medical journalism which can only be surpassed when medical journals are issued more frequently than once a week.

CLINICAL REPORTS.

The winter campaign of medical instruction having fairly opened in our large cities, we shall devote more space than we have of late, to clinical reports; and it is our intention that these shall be select and valuable. These reports as published in this number, will bear critical examination.

MORTALITY OF CITIES.

We are making arrangements, which are nearly completed, for publishing in tabular form regular monthly reports of the mortality of about a dozen of the principal cities of the United States. These reports, which will cost a great deal of labor, will be very valuable, and unlike anything that has ever successfully been attempted before in this country. We are assured of the co-operation of properly qualified physicians in the different cities.

☞ Have the Medical Colleges of this city issued their announcements for the lectures of 1859–60? We have not had the pleasure of seeing any; and had to base our information in regard to the colleges, for the "Students' number" on what we could gather from the last year's announcements.

☞ We would call the attention of our readers to Mr. Daniels' advertisement in regard to Anatomical Preparations, &c.

☞ *Copies of this journal can be obtained of the Janitors of the Medical Colleges in New York.*

Correspondence.

Paris, August, 1859.

Musée Dupuytren.—This museum is situated in an old convent in the Rue de l'Ecole de Mèdecine. It is closed from September until November, but, by feeing the porter, visitors can obtain admission. The wall opposite to the entrance was covered with large placards, announcing private lectures and clinics in the Medical School to which the museum is attached. M. Dupuytren, whose name the Museum bears, left 200,000 francs for the endowment of a professorship of pathological anatomy, but at the suggestion of M. Orfila, Dean of the Faculty, this sum, with another, the amount not stated, was employed in founding a museum of morbid anatomy. In the centre of the room occupied by the museum is a marble bust of Parè, and around the room a number of portraits, while over the door is placed a plaster bust of M. Dupuytren. We could not but regret that it was not a more durable statue We were disappointed in not finding a catalogue, a positive necessity in such institutions. Most of the diseased specimens are accompanied with a short history, but the only really fine preparations are of diseased skin, modeled in plaster, finely colored from nature, and carefully classified. There are some 100 models from l'Hôpital St. Louis, service of M. Cazenave; some large casts of syphilitis, equally good, from l'Hôpital du Midi, service of M. Ricord. There are also some good models of defective formation, hare-lip, etc.; and a number of interesting specimens of diseased bone. Many of these preparations require change and renewal.

Aug. 20th.—Visited to-day *L'Hôpital Beaujon*, Rue de Faubourg St. Honore. This is a large and well ventilated hospital, heated by dry heat. Each ward contains a large stove, the chimney of which connects with a central shaft. The situation of the building is elevated. The original building was 96 feet on the street, and 44 in depth, consisting of ground floor and three upper stories, with large windows that admit an abundance of light and fresh air. Four pavilions, with gardens, have been added. The floor of the wards is waxed and kept very clean. They contain 438 beds, the bedsteads being of iron, with white curtains, giving the whole an exceedingly neat and comfortable appearance. The annual average of patients is 5,100, and the mortality stated to be 1 to 9.45. Their rules are very strict in receiving visitors, and the friends of the patients are examined, to prevent their taking improper articles, such as wine and spirits, etc., into the wards. The physicians are Drs. Goupil, Bihier, Gubler and Velpeau; Surgeons, MM. Malgaigne and Hagnier.

Aug. 22d.—In company with Dr. Coleman, of Georgia, visited the new Hôpital Lariboisiere, St. Lagare, opened in 1854. It was begun under Louis Phillippe, and was named after Mme. Elisa Roy, Comptesse de Lariboisiere, who, at her death, left 900,000 francs towards the erection of this magnificent building, which cost 8,000,000 francs. The ground plan is rectangular, and covers 34,395 metres. A fine colonade fronts on a spacious court, and incloses nine uniform pavilions, separated from each other by smaller courts and gardens. Each pavilion has two stories, besides the ground floor, and fifteen windows in front. Two of these pavilions are occupied by the offices and lodges of the officials; the others contain the wards, dispensary, bathing establishment and laundry. The chapel, which is of the Doric order, is at the end of the court facing the garden. It contains a fine monument to the Comptesse Lariboisiere. The hospital contains 612 beds. Twenty-four sisters of charity of the order of St. Augustine attend to the nursing of the patients, with numerous male and female assistants. There are in attendance six physicians and two surgeons, as follows: Drs. Oulmont, Moissenet, Poideaux, Bourdon, Sardene and Hèrard; Surgeons, MM. Voellemier and Chassaignac. M. Chassaignac lectured, and operated with his favorite instrument, the écraseur. The first case operated upon was a man aged about 35, thin and feeble, with double *fistula in ano*. M. C. administered chloroform. The amphitheatre is a very good one, and there were present from 75 to 80 medical men. He had some twelve or fourteen assistants.

He then exhibited a man aged about 60, who had undergone, with success, resection of the tibia. The man was able to walk about.

3d Case.—Extensive tumor of the anus, which, after the administration of chloroform, he endeavored to pull out; the patient resisted very much, but after continuing the administration of the chloroform, he withdrew it by the double hook, and applied the écraseur, and in a few seconds it was off; no hæmorrhage. The patient had a chill before leaving the operating room. When removed, the tumor was not one-half its original size, the blood having been pressed out.

4th Case.—Extensive tumor, involving the lower maxillary bone in an old feeble man. The tumor projected from his mouth, and involved the skin of the face. After dividing the skin at the median line, down to the bone, he dissected back, and then passing a trocar and canula behind the bone, through the canula introduced a chain-saw, and cut through the bone. He then disarticulated the jaw-bone and separated it; after which he cut through the tumor by means of the écraseur. There was not as much hemorrhage as in the ordinary method of operating.

Case 5th.—He then showed a child in whom he

had divided the extensor tendons of the hand, with success.

Case 6th was another one of diseased lower maxilla, in which he removed, by the same means, a portion from its centre, with success. In neither of these latter operations did he employ chloroform. He exhibited eleven cases, and operated upon nine. In two cases he had diseased mammary gland. One he removed by his favorite method, not successfully, and the second by means of the bistoury.

M. Chassaignac is a fine looking man, with a very intelligent face, active in all his movements, not tall, with a clear, distinct articulation and voice, and very pleasant manner with his patient. His hospital and clinic is one of the most popular in Paris, and his Monday of each week is looked forward to with much pleasure by the medical men who visit Paris. The week previous he removed a cancerous tongue; but first making an opening under the chin, and introducing a tube of gum elastic, so as to produce, before operating, his system of drainage. He also removed a large portion of the femur, by means of the chain-saw, resecting it. The man was still living on Monday, the 21st. In a case of abscess of the chest from diseased rib, he passed a silver tube and trocar, and introduced his piece of gum elastic, and in this way removed the pus without opening the abscess in the usual way. L. T.

Medical News.

Drs. H. Hartshorne and *Laurence Turnbull* of this city have just returned from Europe, in improved health, and prepared to enter with vigor upon their winter's labors.

A writer in the *Medical and Literary Weekly*, who, no doubt, is hunch-backed, says, that "if a man's shoulders are humped, it is a sure sign that they have some weight of brains above them, to press them down"

He gives in illustration "the wheat field, where you will see that those stalks whose heads are heavy with the precious grain, are all bowed down; but those only whose heads are light, and contain only chaff, stand erect!"

Mr. T. Deville, of London and of the *Ecole Pratique* of Paris, has just been appointed Professor of Anatomy at Lind University, Chicago, U. S.—*Gazette Hebdomadaire, August* 12.

The Cholera is spreading along the shores of the Baltic.

Anæsthesia of Bees.—Chloroform has been effectively used in removing honey from bee-hives in England and this country.

The vapor of chloroform is blown into the hive until the bees are asleep, when they can be handled with safety, and in a few hours they are again as lively and busy as ever.

The Cholera in Germany, the *Lancet* says, is still increasing. In some villages half the inhabitants have been carried off. Agricultural operations have been suspended, and the cattle let loose in the fields, there being no one to attend to them.

Of all the infants born in England, 40 per cent. die before they are three years old.

The Military Hospital at Nebley, near Southampton, England, will be, when finished, the largest hospital in the world. It will accommodate between one thousand and two thousand patients.

Excitement from Fast Traveling.—Lord Shaftesbury says: "I have ascertained that many persons who have been in the habit of traveling by railway have been obliged to give it up, in consequence of the effect upon the nervous system."

In our own experience of railway traveling, the dozy, listless appearance of the travelers would incline to the opinion, that it is rather productive of tranquility than nervous excitement. Of course, this opinion does not include railroad collisions, etc.; when the individual, if not entirely annihilated, might probably be somewhat excited. But for that excitement the fast traveling could not be blamed, as it would then be at an end. The passenger would then be like the Irishman, who said that he was not hurt by the fall, but "stopping so quick."

Dentists' Titles in England.—The right of Dentists, in England, to call themselves *Surgeon*-Dentists when not graduates of a College of Surgeons, is being tried in accordance with the new Medical Act. The following is the view of the subject presented by the Committee of the Registration Association:—

"The word 'Dentist' means an operator on the teeth. Surgeon-Dentist implies that, in addition to the occupation of 'Dentist,' the party has the statutory qualification of 'Surgeon.' It is penal to use the prefix 'Surgeon' unless in possession of the legal qualification conferring such title."

Wills Hospital.—At a late meeting of the Managers of this institution, Dr. T. G. Morton was appointed one of the Attending-Surgeons.

The average weekly mortality of London is eleven hundred and two deaths, or about one hundred and fifty-eight daily.

Philadelphia Hospital.—The subject of Dr. Ludlow's introductory lecture to the Clinical course, will be the History of Clinical Medicine.

But little on the interesting subject of the development of the Clinic has been written, and in the hands of this accomplished scholar it will certainly be made very attractive.

The lecture will be delivered on Wednesday next, the 12th inst., at 9½ o'clock, and will be followed by the Surgical Clinic.

Counter-poisoning.—An exchange paper says, that a man who was bitten by a copperhead snake, in Rockbridge County, Virginia, died in twenty-four hours. A quart of whiskey was administered to him, and the attending physicians attributed his death to the *remedy*.

Flavoring Tobacco, so as to make the ordinary qualities resemble in perfume and taste the finest Cuban article, has been successfully accomplished by Professor Liebig.

The *Lancet* says, that connoisseurs have not been able to distinguish tobacco thus prepared from the best imported.

Publications of the Syndenham Society.—The following works are announced for the year 1860:—

"Clinical Memoirs on Abdominal Tumors and Intumescence," by Dr. Bright; edited by Dr. Barlow. A Year-Book for 1859, on Anatomy and Physiology, Medicine, Surgery, Diseases of Women and Children, Forensic Medicine and Toxicology. Frerich's "Clinical Account of Diseases of the Liver." Hebra's Atlas of Illustrations of Skin Diseases; and Professor Simpson offers to edit a reprint of Smellie's Midwifery.

The Workhouse, as pictured by Dickens, is, at even this time, no myth.

The *Medical Times and Gazette* says, that a visitor at the Cork Workhouse was shocked at the appearance of the children, of whom a large number are maintained in the building. Upon investigation he discovered that an enormous proportion were afflicted with scrofulous diseases; many of them dying prematurely; others becoming blind; and those who survived, dragging on a miserable and feeble existence to which death itself would almost appear preferable.

Dr. Callanan says, when examined on oath before the Poor Law Inspector: "On the day I arrived, it was the hour the boys were at dinner. I examined the food, and after their meal I inspected it. I asked for a bowl of the soup; I examined and tasted it, and I found it almost perfect water. I asked of what it consisted, and was told of rice, oatmeal, and vegetables; some green leaves of leek were in the soup. I went along the form while they were at the meal, and I found some of the porringers untouched. I asked them why they did not take their soup, and they said they could not drink it. I saw the bread, and thought it very inferior; it smelled sour, and was heavy and gluey."

On this evidence, says the above journal, Mr. Arnott makes the bitter remark, that in Dickens' "Oliver Twist" the workhouse-boy astonishes his superiors by *asking for more*,—but in the case of the Cork Workhouse-boys, they prefer starvation itself to eating the food placed before them.

Gallows Confessions.—We have before noticed the heresies which afflict all varieties of quacks, when they attempt to band together for the purpose of attracting public attention and mutual admiration. This has been evident in such meetings as the late Homœopathic Convention, in which it was admitted that a portion of the order were inclined to merge into "Eclecticism." And now a noted homœopath, Dr. Hering of this city, whom we have looked on as the very personification of homœopathy, and who, we supposed, relied on it with the devotion of a fanatic, yields with an honesty not to have been expected, the following acknowledgment, which we copy from an article by him in the *American Homœopathic Review*.

He says: "We all know that the numbers in our homœopathic ranks are not lessening; but it is the general observation that the number is year after year increasing, who, instead of deriving benefit from homœopathy, are made incurable by so-called homœopathic practitioners."

Glycerine was successfully used to prevent the pitted cicatrices from small-pox pustules, during the recent epidemic at Berlin.

Army and Navy Intelligence.—We are indebted to the *Century* for the following items:—

Assistant-Surgeon J. Leatherman, Medical Department, has been assigned to temporary duty in the City of New York, as Assistant to the Medical Surveyor at that station.

Assistant-Surgeon S. W. Crawford has been ordered to repair to New York city, thence to report by letter to the Surgeon General of the Army.

Dr. G. I. De Camp, Medical Department, has returned to St. Louis from sick leave, looking much improved in health. He resumes his duties as Staff-Surgeon in that city. Dr. J. H. Bailey, who acted in his absence, returns to his station at Jefferson Barracks.

Leave of absence for thirty days, for the benefit of his health, has been granted to Assistant-Surgeon W. H. Babcock, Medical Department.

Surgeon D. S. Green and Assistant-Surgeon Bennett Green have been ordered to Panama, by the steamer from New York of Oct. 20, to relieve the Surgeons of the steam-frigate Saranac.

Assistant-Surgeon Richard C. Dean has been ordered to the U. S. steamer Crusader, which has just left this port for the Gulf of Mexico.

Passed-Assistant-Surgeon Lowber has been ordered to the steamship Narragansett, at Boston, and Assistant-Surgeon Laws has been ordered to report for duty at the Navy Yard of this city.

The Great Eastern steamship has two Surgeons appointed to sail in her. . . . J. B. Lippincott & Co. propose publishing a Manual of Spherical and Practical Astronomy, to be prepared for the press by Professor Wm. Chauvenet, and illustrated with steel and wood engravings. . . . The southern cities have thus far escaped epidemics of yellow fever, and it is now too late in the season for any extensive prevalence of it. . . . A Society of Dentists for the northwest of Germany has been formed. . . . A medallion portrait of Orfila, the Toxicologist, has been placed as a tablet on the house in which he was born, at Port Mahon, Balearic Islands. . . . A Clinical History of the Eastern Royal Maternity Charity, during 1858, has been published. . . . A book has been published in England with the title of "Chloroform, its Use and Abuse, by One who has been Thirty-three Times under its Influence."

The first case of yellow fever for the season was reported at New Orleans on the 3d inst. (Rather late in the season for a "successful run.")

To Correspondents.

B. S., Ohio.—Propylamin is prepared by B. J. Crew & Co., chemists of this city. It can be procured from them.

Dr. G., Ala.—Any money is received in payment for subscriptions which circulates at par in the locality from whence it comes. It is a good plan to *paste gold dollars between two ordinary visiting cards*, and secure the envelope well. We then have no discount to pay.

COMMUNICATIONS RECEIVED.—*Alabama.*—Dr. A. M. Graham, (with enclosure.)

Delaware.—Dr. Geo. McC. Miller.

District of Columbia.—Dr. G. M. Dove, (with encl.)

Indiana.—Dr. Calvin West.

Illinois.—Dr. T. D. Fitch, (with encl.)

Maryland.—Dr. James Dwinelle, (with encl.,) Dr. Charles Frick.

Mississippi.—Dr. Robert E. Richardson, (with encl.)

New York.—Dr. Wm. C. Roberts, (with encl.,) Dr. C. H. Shears, Dr. L. Elsberg, Dr. H. B. Horton, (with encl.,) "Gotham," (3,) Dr. J. W. Gray, (with encl.,) Dr. J. M. Newman, Mr. C. W. Polman, Dr. Chas. H. Covell, (with encl.,) Drs. Kiernan and O'Meagher.

Ohio.—Dr. A. H. Stephens, (with encl.)

Pennsylvania.—Dr. Jos. L. Cook, (with encl.,) Dr. J. W. Rugh, (with encl.,) Dr. W. W. Wick, (with encl.)

Virginia.—Dr. J. L. Dorsett, (with encl.,) Dr. P. M. Cline.

Office Payments.—Dr. J. A. Meigs, Dr. A. Foulke.

MARRIAGES.

EDWARDS—WALLER—At Bridgeport, Conn., Sept., 29, by the Right Rev. Bishop Williams, Wm. G. Waller, of Baton Rouge, La., to Harriet S., only daughter of Surgeon D. S. Edwards, U. S. Navy.

PETERS—STOUTENBOROUGH—In Brooklyn, Sept. 28, by Rev. Wm. H. Lewis, D. D., De Witt C. Peters, M. D., to Miss Emily G. Stoutenborourgh, of Brooklyn.

SQUIRE—FLEU—On the 22d September, by Rev. John Rodney, Dr. Wm. H. Squire to Mary E. Fleu, both of Germantown.

WISTER—BUTLER—Oct. 1, 1859, at the Church of the Epiphany, by Rev. J. W. Cracraft, Miss Sarah Butler to Dr. Owen Wister, both of Philadelphia.

DEATHS.

HANDY—At Washington city, Sept. 29, Dr. Charles W. Handy, of Maryland, of consumption, in the 49th year of his age.

JACKSON—Sept. 9th, at Hopkinsville, Ky., of consumption, John H. Jackson, M. D., in the 31st year of his age.

LIVINGSTON—In Cincinnati, Sept. 27th, Dr. John H. Livingston.

MCBARRON—In New York, Sept. 30, Dr. Patrick McBarron, aged 36 years.

MAYHEW—Sept. 20, in New Bedford, Mass., Julius S. Mayhew, M. D, æt. 68 years.

PANCOAST—On Seventh Day afternoon, the 1st inst., George A. Pancoast, son of Dr. Joseph Pancoast, in the 28th year of his age.

HIRST—In Galesburg, Ill., Sept. 1st, Dr. C. J. Hirst, late of Altoona, Pa., in the 40th year of his age.

BUTLER—At Davenport, Iowa, on Saturday, Oct. 1st., Oliver H. Butler, M. D., eldest son of Elias Butler, of Hyde Park, New York, aged 24.